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THE FIRST CROSSING OF ANTARCTICA: *A paper read at the Evening Meeting of the Society on 30 November 1936, by*

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IT is with a deep sense of the modesty of my own endeavours that I appear before you to-night to tell you something about my recent flight across Antarctica. It is twenty-three years since I last was in London. I wanted to become an explorer, so came over here to buy the necessary instruments, which at that age I thought would be the necessary qualification, but found that I must also take instruction, which I did, under Mr. Reeves, of your Society. Strangely enough, the pocket compass with which I practised route-surveying on weekly excursions about the suburbs of London I carried and used on my flight across Antarctica last season. I prize it highly because, after all, it remains the memento of a visit that was to decide my whole future career. Had it not been for that beautiful Emperor Penguin which I visited weekly at the London Zoo and the memorial service which I attended at St. Paul's in February 1913 for Captain Scott and his gallant comrades lost in the Antarctic, I should probably never have chosen the polar regions as my field of endeavour. But so deeply stirred was my imagination by what I had seen and heard that the die was cast before I left London.

The dreams of youth are long, long dreams; yet despite all disappointments and setbacks that were to ensue the purpose held, until a chance meeting with Amundsen made possible an aeroplane flight over the polar sea. My meeting with Amundsen was in 1924. Our flight the following year to within 120 miles of the North Pole, from Spitsbergen, was the first penetration of the polar regions by means of an aeroplane. It proved the value of aircraft as a future means of polar exploration.

After my flight with Amundsen in the *Norge* from Spitsbergen in 1926, over the north polar basin to Alaska, restlessness and desire nagged me until I was able to settle on the last great adventure of south polar exploration: the crossing of Antarctica.

A crossing of the area between the Ross Sea and the Weddell Sea seemed to offer the best possibility of solving the major problem in the Antarctic. Does the Andean massif which dips at Magellan Straits, rises to form the

grand mountain chain of Graham Land, and dips at what is believed to be Stefansson Strait, rise again in Hearst Land and continue until it joins the polar plateau and the mountains of South Victoria Land; or, if it does rise in Hearst Land, does it dip again to form a depression or a below sea-level channel connecting the Ross Sea with the Weddell Sea?

Numerous plans and many preparations had been made to carry out a trans-Antarctic journey. Shackleton in 1914 lost his ship before he reached the starting-point. Wilkins was foiled two years in succession by failing to find a suitable aeroplane field from which to start. Watkins, after much preparation and endeavour, failed to get started from England. Riiser Larsen was carried away from the barrier edge by the breaking up of the ice, and did not get started on the actual journey.

By the time I was able to turn my attention to the problem a great advance had been made in the machinery for aerial transportation, and I believed that it was no longer necessary to risk the lives of many men who by their sheer physical endeavour would fight their way slowly through storms, starvation, blizzards, and snow blindness, struggling for many weeks against tremendous odds across what might prove to be a monotonous stretch of sastrugi-featured snow: or, if mountains were found to exist between the Weddell Sea and the Ross Sea, find themselves in a maze of overhanging glaciers, steep-sided valleys, and faced with unscalable cliffs such as could be seen in Graham Land.

I myself would have preferred to have been with the vanguard of polar explorers, and am happy in the knowledge that neither the North Pole nor the South Pole were first humbled by conquest by aeroplanes. Nevertheless it would not do for us to lag behind the times. Change is the law of the world itself.

> "The hills are shadows, and they flow
> From form to form, and nothing stands."

says Tennyson. And so our method of approaching the mystery and romance of Nature's last stronghold against man's invasion had to change.

My own experience with aeroplanes in high latitudes had given me some knowledge of the possibilities of aircraft in polar conditions. I knew that low temperatures would not be a great obstacle to flight. I had experienced the fact of landing far from my base and setting out again for a safe return. I knew that aircraft engines in 1932 were reliable for many hours' service without overhaul, and I had studied carefully the Antarctic conditions as far as they might affect the use of the aeroplane.

In spite of the general impression created by the books on Antarctic travel, which are filled with descriptions of bad sledging trails, hazy outlines, sudden blizzards and hard-lipped sastrugi, I found that even the written accounts of previous expeditions really encouraged the belief that machines might be landed safely on most of the surfaces encountered in the past. A thorough search through the diaries of Scott, Shackleton, Mawson, and Amundsen revealed that the surfaces they found on their sledge journeys, both on the Ross Barrier and on the polar plateau, would afford reasonably good landing fields for modern aeroplanes, provided of course that the weather was such that these surfaces could be seen.

For instance, in Scott's published diary of his journey to the Pole he

mentions soft snow, heavy and rough surfaces, and some surfaces distinctly good, but he actually mentions sastrugi on five days only during the thirty days it took him to travel from the edge of the Barrier to the foot of the Beardmore Glacier which leads up to the polar plateau. Only on one occasion did he mention sastrugi 12 inches high, and then they were "widely dispersed." The other sastrugi seen must have been lower and probably would not have interfered with the safe landing of an aeroplane. From this it was clear that only a small percentage of the actual Barrier surface would be unsuitable for forced landings.

On the way up the glacier Scott mentions sastrugi "not more than three inches high." There were crevasses and undulations and soft spots of course; one would expect to find these in the fairly steep-sloped area leading up to an altitude of 9000 feet. But it was hardly necessary to consider the glacier surface in relation to Antarctic flight, for any aeroplane flying over them would, in case of engine failure and a forced landing, be able to glide above the glacier surface to the lower level of the Barrier.

When once over the plateau and beyond the "third degree" camp, Scott found the surface difficult for sledging, but not until after five days' marching did he come to sastrugi which were rough and confused—it was so rough in fact that Scott decided to abandon his skis, which he had used up to that time. But the rough condition was limited in area—a cross-section of probably less than 5 miles; for when they came to the end of the sastrugi Scott says they went back for the skis, a trip which resulted in a delay of only an hour and thirty minutes. Onward to the Pole, and between the dates January 7 and 17, sastrugi are mentioned only four times, and then only in relation to the general direction of the winds. He refers to rough surfaces only once, on the 15th, near the Pole, and there the sledges "bumped over the ridges."

In Amundsen's account of his journey from the Bay of Whales to the Pole he mentions sastrugi only five times, and only when on and near the glacier, in the draw of the mountains, did he find the surfaces exceedingly rough.

The general description of the surfaces encountered by Sir Douglas Mawson and the members of his parties was not so encouraging; but then Sir Douglas was travelling in comparatively low latitudes, between 69° and 70°, and his route lay over the sloping ice-sheet not far from where it meets the open water: a condition likely to induce high winds, crevassed areas, and sastrugi. Mawson's Magnetic Pole party led by Bage travelled farther from the continental edge, but was not more than 120 miles from the sea. Bage mentions sastrugi only twice during the first 75 miles of travel, and lists the snowy wind-drifts only once as being 1 foot high; the others must have been lower. Later on he remarks on sastrugi 6 inches high, then for three days very few. After that they travelled for five days over fairly good surfaces, then he mentions "some old sastrugi," then "surface smoothly polished."

Notwithstanding the difficulties which were actually encountered by Mawson's sledging parties in the Adelie Land area, Bickerton did use an air-screw tractor, in reality an aeroplane minus its wings, and taxied it several times over a route 10 miles long. The surfaces on any part of that route if suitable for taxiing must have been suitable for an emergency forced landing.

It is only logical to assume that conditions inland from the Adelie Land

coast would be somewhat similar to those found by Scott and Amundsen farther south. Therefore, in spite of the general impression, and after a careful study of all conditions met by sledging parties, it seemed that on a flight across Antarctica it would be possible to find at least as many emergency landing fields as one would expect to find on any trans-continental flight before the development of air-routes.

The question, then, resolved itself not into one of safe landing fields, but whether one could distinguish a safe landing field from the air. There is no doubt about the difficulty of distinguishing the type of snow surface when the sky is clouded or during snowdrift and blizzards, but it is not difficult in sunshine. To avoid danger it would be necessary to fly only in clear weather. So the weather problem was the greatest to be faced in relation to the use of aeroplanes in the Antarctic. It was in fact almost great enough to prohibit any attempt to make a trans-Antarctic flight. Storms without warning had frequently overtaken foot travellers, but it was obvious that when flying it would be possible to see the approach of a storm at a greater distance than when on the ground, and also possible to turn away from it if necessary. The thing to do if a storm barred the way was to land and wait for clear weather.

But the high winds had wrecked tents and almost smothered polar parties, and even before my plans were made had ripped one of Admiral Byrd's planes from its lashings and carried it half a mile away. This was a high-winged type of plane, difficult to anchor, and it had been anchored to shallow snow on a glazed ice surface. But even had it been firmly made fast its high wing would have left it at the mercy of the wind.

It was certain that insufficient data could be obtained from the Antarctic to allow reliable forecasting of the weather which might be encountered on the entire route between the Weddell Sea and the Ross Sea, and it was necessary, if I should attempt to fly over that area, to prepare to land in the face of bad weather. This plan required special attention to the type of plane to be used. First I would need a plane with high speed, so as to limit the time risk as far as possible, and to overcome the high winds which would surely be met with in clear weather. The machine would have to be of a type which could be made fast safely to the ice or snow while riding out a blizzard. A low-wing type would serve that purpose, and one without struts or wires attached to the landing gear would best facilitate the lowering of the skis into channels dug in the snow and the digging out of the plane from the snow-drift when the blizzard was over.

In 1931, when my plans were first announced, there was no plane of the type I required, but a canvass of the possibilities showed that a development of the Northrop Alpha plane, a low-wing all-metal machine with cantilever streamlined landing gear, would best suit my purpose. At my request the Northrop Airplane Company built a special machine, the *Gamma*, for my service. It was large enough to carry sufficient fuel for a range of 5000 miles if nothing but fuel and oil was to be carried; but naturally some of my load would consist of equipment and supplies. I intended to take enough for two months, so as to allow for repeated landings, and enough to serve even if we had in emergency to abandon the plane and walk to the nearest area which

6. 21 November 1935. 12.19. The Eternity Range as seen on the first flight

3. 23 November 1935. 12.25. East coast of Graham Land seen to right

7. 23 November 1935. 12.38. Left towards Cape Eielson and beginning of Eternity Range

9. 23 November 1935. 12.41. To right, across entrance to Stefansson Strait

11. 23 November 1935. 12.45. The same a little later, looking farther west up the strait or valley

A. Probably the opposite face of the features in middle distance of No. 11, taken on flight November 21

would provide food in the form of seals and penguins. The fuselage of the Northrop *Gamma* was large enough to accommodate a pilot and navigator, a sledge, skis and snow-shoes, sleeping-bags, tents, engine covers, and heating stoves required for conveniently starting the engine in cold weather. The machine was of course equipped with a two-way wireless as well as an emergency radio outfit. The final design incorporated a 600-h.p. Wasp engine, giving a possible speed of 215 miles per hour. It had sturdy, short, and wide skis made of wood, sheathed with metal. The skis were interchangeable with wheels and pontoons, so that we might use the machine on any type of surface. A unique feature was the flaps, a very new feature in 1932, which permitted us to land at the comparatively low speed of 50 miles an hour and take off in a short distance.

After the aeroplane was selected it was necessary to find a ship which would accommodate the machine in the hold for safe transport through the stormy waters we should cross to reach the Antarctic. My first plan, based on the assumption that in the Ross Sea, at the edge of the great Ross Barrier, was the only place where I could be certain of finding an unloading point for the plane on skis, was to fly from the Ross Sea to the Weddell Sea and return on a triangular course.

My ship would have to brave the stormy areas south of New Zealand, and have sufficient range to journey round the Pacific sector of the Antarctic ice edge to pick me up, should I be compelled to land on the Weddell Sea side or abandon the plane en route and walk to some part of the coast. I finally selected a staunch, single-deck, motor-driven Norwegian fishing-boat of 400 tons. She was built of Norwegian pine and oak in 1919. I sheathed her with oak and armour plate for service in the pack-ice. Her engine was of the semi-Diesel type, and I installed tanks for fuel sufficient for cruising 11,000 miles at a speed of 7 to 8 knots. I named the ship the *Wyatt Earp* after an unbelievably brave frontier marshal who more than any other man of his time typified the empire builders of the western United States.

The *Wyatt Earp* could carry supplies for two years as well as the aeroplane in the hold, where it was well protected from the weather. This left our decks clear, except for the explosive gasoline, which was carried in drums lashed to the deck rails. There was room in fact to have carried another plane on deck, but I finally decided against taking a duplicate machine for two reasons. First by exercising infinite care with the machine while preparing it for flight, and by flying only in fine weather, landing only in clear weather, and in time to lash it down before being overtaken by storm, I could be reasonably sure of safety from damage. A skilled pilot, landing under such conditions, would protect us from the danger of accidents to the personnel and leave us in a position to walk away from the machine if it was necessary to abandon it. Secondly, two aeroplanes would have meant two or more pilots and other additional members of the expedition, and in all an extra cost far in excess of the actual and initial cost of the machine.

Economy has not always been the first consideration in polar exploration; and while it is necessary to spare no expense in providing adequate equipment, there is no reason why expeditions should be absurdly expensive or luxurious. It is impossible of course to value discovery in dollars and cents,

but all attempts at discovery should be organized with some consideration for the magnitude of possible results compared with the amount of money involved. In this relation the use of aeroplanes has made it possible to lower the cost of exploration and discovery.

An expedition equipped for flying needs less personnel, and can expect to cover miles for less expenditure of energy and money than could be done when using the dog-team method. For detailed scientific research the dog-teams may be necessary, but my efforts were to be purely in the nature of discovery, to open the way for future research to follow. To achieve my purpose I believed that by carrying sufficient food and equipment in the plane, we could if required spend several weeks on the journey, and if by chance our plane was wrecked we could either hold out until my ship could return to civilization, pick up another plane, and come to our rescue, or else meet us at a predetermined point to which we might walk, and where we might find additional native food.

Adequate radio precaution would assure us of communication with our base at all times, and to cover this I prepared not only for two-way radio communication from the plane but carried as well a complete engine generator set for use when the plane was on the ground, and a complete hand-driven set for use when sledging. This, we believed, would take care of all emergencies. But we were wrong, for during our flight a terminal inside the sending set, and which we could not reach, burned out. While we were on the ground between flights we maintained the prearranged schedules three times daily, but the signals sent out by the engine generator set were never heard. The oil in the hand-driven generator froze stiff and stripped its gears.

Believing that we had ample fuel we did not conserve our supply either when climbing to high altitudes over Hearst Land or during the various landings, and this brought us to a landing out of fuel when within 16 miles of our goal. There was no useful radio gear at Little America, and with a frozen foot and considering the difficulties of crossing the terribly crevassed area and pressure ridges which lay between Little America and our plane, I did not think it possible to haul equipment and fuel sufficient to bring the machine to Little America, where in time it might have been possible to repair the wireless set. So after the set failed, when we were about half-way across the Antarctic continent, we were out of touch with our base. We did however pick up time signals on three separate days from a station at Buenos Aires, and this made it possible for us to check up on our chronometers and establish our positions.

With the exception of our wireless, our equipment served its purpose admirably. There was no difficulty with the aeroplane equipment because of the cold. Wherever we landed on the plateau surface the snow was smooth and hard packed. The skis sank less than an inch, and we had no difficulty in landing or taking off, or in securing our plane so that it was safe during the blizzards we experienced. This shows that a carefully selected machine and suitable equipment, in the hands of a skilled pilot, will serve for preliminary reconnaissance in the Antarctic. Even in the mountainous areas we crossed in Hearst Land there appeared to be many places, which would be difficult to reach by dog-team, where we could have landed safely.

21. 23 November 1935. 14.37. To right, over un-named mountains

23. 23 November 1935. 14.45. Continuation of mountains to right, overlapping No. 21

24. 23 November 1935. 14.53. Continuation of mountains to right, overlapping No. 23

25. 23 November 1935. 15.00. Continuation of un-named range to the left

26. 23 November 1935. 15.05. Continuation to left overlapping No. 25

39. 23 November 1935. 19.30. The Sentinel range, to the left

An aeroplane can carry more and bring back more specimens than a dog-team could haul, and although much time might pass while waiting for reasonably good weather, the speed of travel when the weather is good and the excellent visibility to be had when travelling by plane more than compensate for the delay. Therefore I think that with the aeroplane we can reveal the last remaining unknown regions on the face of the earth.

So much for our plans and the way in which they failed in certain points. The details of the flight have appeared already in the publications of American Societies, but a brief summary of them will still be in place here.

In 1933 I had planned to make the flight in the opposite direction, from the Bay of Whales to the Weddell Sea, and in January 1934 had landed the aeroplane on the bay ice which, after a successful trial flight, broke up in a gale and crushed the machine so extensively that we had to abandon the attempt.

In September 1934 we were back in New Zealand ready for another attempt; this time to make the flight from the Weddell Sea to the Ross Sea, because an earlier start was possible in that direction owing to earlier break-up of the pack-ice about Graham Land. But the weather was altogether against us, first at Deception Island and later at Snow Hill Island on the east coast of the Archipelago, where we found a suitable flying base. We made a start on 3 January 1935, but were soon driven back by bad weather. In the whole of that season, and we were there for three months, we had less than twelve hours of flying weather. Returning from Snow Hill Island we were caught in the pack and got free with difficulty, and for the third attempt we chose a safer base, Dundee Island, some 80 miles north of Snow Hill, which we had marked on the return from the previous attempt. The *Wyatt Earp* reached Dundee Island again in November 1935 with Sir Hubert Wilkins and five others who had been on all three expeditions. For pilot on this flight I was fortunate in obtaining Mr. Herbert Hollick-Kenyon, who had obtained leave from Canadian Airways; he had much experience of flying in sub-arctic conditions.

At the summit of Dundee Island, about 500 feet above sea, there was an almost unlimited snowfield with an excellent take-off slightly downhill. Supplies and fuel were hauled up on sledges and personal equipment carried by the plane during test flights. The weather was favourable and we took off on November 21 in clear weather for what we hoped would be the main flight, but after about 600 miles our fuel gauge clogged, so that we were forced to return, and landed after ten and a half hours in the air. We had realized from our experience on the first two expeditions that the only way to fly in the Antarctic is to start in good weather, and if it turns bad to be prepared to land and await the return of better conditions. So this time we took no meteorologist on our base staff, feeling that it is impossible to forecast in the Antarctic.

Next day the machine was refuelled and the engine tuned up again by the mechanics. The weather promised to remain clear, and Hollick-Kenyon and I were called at 02.00 on the 23rd. (All dates and times are Greenwich Civil Time.) We ate a hearty breakfast and then dressed in heavy clothing,

with snow-shoes. We purposely made slow time walking the 5 miles to the plane, because we did not wish to get our clothing damp with perspiration before taking off. After two hours we reached the place where the *Polar Star* lay ready for flight. As Kenyon busied himself with last adjustments I had only one thought: "This time we must make it."

When we took off to the south at 08.04 on November 23 the weather was clear, the temperature −3° C., and the sea a turquoise blue which gave a marvellous reflection on the mountains. By 08.30, as we flew along the coast of Ross Island, we had climbed to 6400 feet and the temperature had dropped to −10° C. Weddell Sea was quite open for the first 300 miles—unusual in the Antarctic springtime. For 600 miles we flew along the eastern coast of the Antarctic Archipelago, until we came to the frozen channel which we identified as Stefansson Strait. It appeared to be not more than 3 miles wide, much narrower than is shown on the map of the American Geographical Society, and we could not see far enough to determine whether it actually connected the Weddell and Bellingshausen Seas, or was merely a deep fjord, though we had risen to more than 13,000 feet.

So long as we had landmarks for checking the plane's ground speed and position we made careful notes of dead reckoning, and found that our ground speed was lower than expected, but was 120 m.p.h. or more. By 09.30 we noted that there was some wind and later that we were drifted too far to the east, and our course was altered to allow for this.

The low, black, conical peaks of Cape Eielson rose conspicuously on our left, and with keen curiosity we gazed ahead at the great mountain range to be crossed. Bold and rugged peaks, bare of snow, rose almost sheer to some 12,000 feet above sea-level. Impressed with the thought of eternity and our insignificance, I named the new mountains the Eternity Range, and the three most prominent peaks on our right Faith, Hope, and Charity, because we had to have faith, and we hoped for charity in the midst of cold hospitality. They were in striking contrast with the flat low peaks of the Antarctic Archipelago which we had followed south, peaks which dwindled into low isolated nunataks as we neared Stefansson Strait. The range which we were now crossing was loosely formed, with none of the crowded topography of peaks and glacier-filled valleys with crevassed bottoms.

After three hours the mountains beneath us gave place to a vast polar ice plateau from which emerged a few nunataks, the last relics of the mountain chain just passed. We were flying at 10,000 feet, which was the average altitude of our flight. During the first hours of the flight we had constant two-way radio communication between the ship and the plane. But at 16.15 I logged: "Transmitter out of action. Only thing is to go on." We had traversed 1000 miles and were yet 1300 miles from the Bay of Whales. We sighted several isolated mountain peaks, but these soon faded out on our right about 16.20. Forty-five minutes later other peaks showed on the same skyline, and in another twenty-five minutes more mountains 120 to 140 miles away appeared on our left horizon, and also a few peaks to the right.

Sun sights taken at 16.53 and 18.54 gave a fix which appeared to show that we were more than 200 miles west off our course: as will be explained later the bubble sextant had got badly out of adjustment. At 17.00, when by estima-

tion we had passed out of the Falkland Islands sector, I logged: "Long. 80° dropped American flag and named the land up to 120° west James W. Ellsworth Land. What a thrill ! ! !" One hour and forty-five minutes later we came abreast of a solitary little range about 25 miles away on our left, symmetrically formed, with a central pyramid rising to 13,000 feet. I named it Sentinel Range, and its central peak Mount Mary Louise Ulmer, after my wife.

Fifteen minutes later, and 100 miles distant on the southern horizon, appeared a long, black, flat-topped range which extended visibly through at least 1° of latitude. This looked like the last of the mountains we were to see, for ahead lay only a vast plateau to the horizon. At 20.30 I noted: "No land marks visible. Only a limitless expanse of white." At 20.45 Hollick-Kenyon passed me the following message: "I really have no idea where we are—but our courses carefully steered should put us close in," and it proved in the end that we had remained surprisingly close to our scheduled course, within 45 miles, though our speed had been much lower than expected. We had been in the air nearly fourteen hours; visibility began to get poor, and we determined to land and take sights of the sun for our position, for we had no fuel to spare. We had no knowledge of what the surface might be like, and it was misty on the ground, but we landed safely at 21.55 on November 23, though we crumpled the fuselage in landing.

This was the first of our four landings during the crossing, and twelve of the nineteen hours here were spent in taking observations to check the position of this our first camp, which we will call Camp I. After getting one position line, it was necessary to wait two or three hours to get another line crossing the first at an angle sufficient to give a reasonable intersection. I went out once to get exercise between the observations, but the monotony of the terrible expanse of endless white got on my nerves, so that I was glad to get back into the four walls of the tent. There are twenty-four hours of daylight in this region at this time of year, and that too wears on the nerves. The temperature was 15° below freezing. During our nineteen hours here we strung up the antenna wires on the bamboo sledge poles, worked the sledging-set transmitter by hand, and kept on sending calls, both general and to the *Wyatt Earp*, but we got only one response during the twenty-two days of our journey across Antarctica, and that was: "We can't hear you." The dead reckoning position of the camp was lat. 80° 28′ S., long. 141° 02′, but our ground speed had been much over-estimated; for the position as determined by our observations was 80° 20′ S., 104° W. And we had not then overcome the trouble with the sextant. When its index error was eventually discovered and the sights recomputed, it proved to be 79° 15′ S., 102° 35′ W. The snow on the high plateau was granular and packed so hard that the skis of the plane made little impression. The surface elevation was 6400 feet, and the plateau extended with slight undulations in all directions.

The Pole lay 750 miles south, Dundee Island 1550 miles behind us, the coast-line of the continent several hundred miles to the north, and the Bay of Whales 750 miles ahead. It was here that I raised the American flag, and so far as that act would allow, claimed the sector between longitudes 80° to 120° W. for the United States, having already in my mind named it James

W. Ellsworth Land after my father. That part of the plateau above 6000 feet I called Hollick-Kenyon Plateau. We set up our balloon-silk tent and took repeated altitudes of the sun with the sextant.

After nineteen hours at Camp I we again took to the air at 17.00 on November 24 in calm weather, but looking thick ahead. We felt we must push on, for our chances of a successful crossing were decreased in proportion to the time we lost at any one place. We soon experienced low visibility, and at the end of a short half-hour we were finally forced to land again, with a ground elevation of 6000 feet. We were surprised at the ease with which we could land or take off on a hard surface. It required no more than 50 yards to rise from the snow when we left the first camp on November 24. This is all the more remarkable since we had no assisting wind, and since we were at an elevation of 6400 feet above sea-level.

At Camp II we waited three days for good weather, trying strenuously and continually, but fortunately unsuccessfully, to fix our position. I say fortunately because the number of observations we made here were useful later in tracing the error of our sextant. After getting only a very rough approximation to our position, we took off in great uncertainty about the precise direction of Little America.

This was on November 27. After 90 miles we landed in a fog, and at 02.30 a blizzard was upon us. On November 28, 29, and 30 we lay all day in our sleeping-bags with drift and gale reaching 50 miles per hour. By November 30 there were huge drifts around the plane, and the cockpits were full of snow. We were unable to get into communication with the *Wyatt Earp*, although on November 30 we got three time ticks from Buenos Aires. We were 600 miles from Little America and probably had not enough fuel left to get there. I consider this stay at Camp III as the low-water mark of our flight.

However with our prospect appearing so dark, our situation was improving. At Camp III we luckily thought of the simple expedient of adjusting the bubble of our sextant on the snow horizon when the index read zero. This showed that the sextant had after the first observation developed an index error of 82 minutes of arc, and that it had been apparently constant. We reduced it to an uncertainty of about 4′, enabling us to fix our position and to set a direct course to Little America. Once the sextant was put in approximate adjustment our navigation problem became a simple one. All observations, except the very first one, were corrected for the determined index error of 82′ and the positions reworked with impressive results.

The second observation taken in the air at 18.54 on November 23 showed that the plane was behind schedule. The big discrepancy in the estimated air speed of 145 m.p.h. and the actual ground speed is accounted for by several factors: (1) The substitution of skis for wheels causing an unexpectedly heavy drag; (2) a slightly crumpled fuselage which altered the streamline and thereby reduced the speed; (3) unexpectedly heavy head winds; (4) low temperatures which reduced the engine power output; and (5) throttling down the engine to save fuel. During the whole mid-section of our flight, from the time we left Eternity Range until we started on the down grade to the Ross Barrier, the prevailing wind blew from the east and south-east. Only twice did we have a north wind, and then only for a few minutes. We never had a

west wind. But these factors hardly account for the reduction of more than 25 per cent. in the speed of the *Polar Star*. The measured speed at the beginning of the flight when the plane was heavily laden, and the known speed of the plane on the last two legs, was relatively much higher, so that an extremely low ground speed of about 92 m.p.h. was made on the first and most dangerous leg of the flight.

On December 1 we spent the whole day clearing snow drifts from the plane, which was one solid block of snow. To crawl in among the controls with a teacup and clear away dry snow as fine as flour was the worst job of all. On December 3 we tried to start up the machine, but the magneto burned out. It looked as though we were 650 miles from the Bay of Whales with no hope of getting there. When the blizzard abated we were able to cut snow blocks to erect a shelter to the windward of our tent. For eight days, until December 4, the storm held us prisoners in the camp. Our only excursions outside during the blizzard were to use the wireless three times daily and to fill our bucket with snow for water. Our food ration was 34 oz. a man each day, but we were not obliged to adhere to the allowance as we ate only twice a day. Even then we were never very hungry. In the morning we had a mug of oatmeal with chunks of bacon boiled in it, milk, sugar, and oat biscuit with butter. In the evening we had a mug of pemmican, oat biscuit, and butter. I thrive on this simple diet, just as in 1925 with Amundsen I never grew tired of our menu of hot chocolate morning and night and pemmican at noon.

One morning we tried unsuccessfully to start the aeroplane motor after warming it for an hour. The situation seemed bad, for we were being buried deeper and deeper in the snow. We decided that we must get out of that hole irrespective of the weather ahead, and after eight days in the blizzard camp we put the canvas hood over the motor and placed the fire pot inside for forty-five minutes, as we always did before starting. Then we cranked the engine. After a couple of weak turns the propeller would stop with a choke. Kenyon connected the stronger radio battery to the starter and had the propeller going in no time. With the plane unloaded we pulled out of the drift, loaded up again, and at 19.20 on December 4 we took off into a sky which was anything but promising. But we had not been flying long before the horizon became clear and the sky took on a beautiful golden glow.

At 23.10 we came down to get a sight, which made Camp IV in 79° 29′ S., 153° 27′ W. It was a beautiful calm night, the boundless snow fields sparkling like diamonds. There was no wind, we had left the high plateau, and were only 145 miles from the Bay of Whales. Once more it was good to be alive. We were now on territory explored by Byrd and all we wanted was to get to our destination.

At 09.00 on December 5 we took off and at 09.50 reached the north end of Roosevelt Island, only 16 miles south of the Bay of Whales, but we did not know this at the time. From the air we saw the ice-free waters of Ross Sea, the goal of my four years of endeavour. At 10.05 the *Polar Star* slackened her speed and came gently to the snow, her 466 gallons of gasoline completely exhausted. Here we made Camp V.

On December 6 we dug trenches to settle the skis in, waiting to walk to

Little America. On December 7 the south-east wind continued, with snow squalls, and temperature round about freezing-point. On December 8, standing on the wing of the plane and looking north-west, we saw among a lot of irregular ice hummocks, that might be snow-covered buildings, what Kenyon thought was a wind-generating tower. Was it Little America? We thought we would trek over on our snow-shoes, but after a two-hour walk we appeared to be no nearer and returned to the plane.

On December 9 we packed our hand sledge with ten days' ration and started off, leaving our tent with the plane, and expecting to find shelter in the huts. We travelled 9 miles of heavy hauling in the soft snow, and as we neared the tower we saw that it was only an ice pinnacle. Being without tent or sextant to fix where we were we had to leave the sledge and return to the plane for both; rested an hour, and got back to the sledge at 03.00 on December 10. We made Camp VI here, and after seven hours' sleep took sights and fixed its position in 78° 38′ S., 163° 20′ W. about 12 miles south of the head of the Bay of Whales. The weather for two days had been perfect, with no wind, the sun shining out of a cloudless sky and the temperature above freezing.

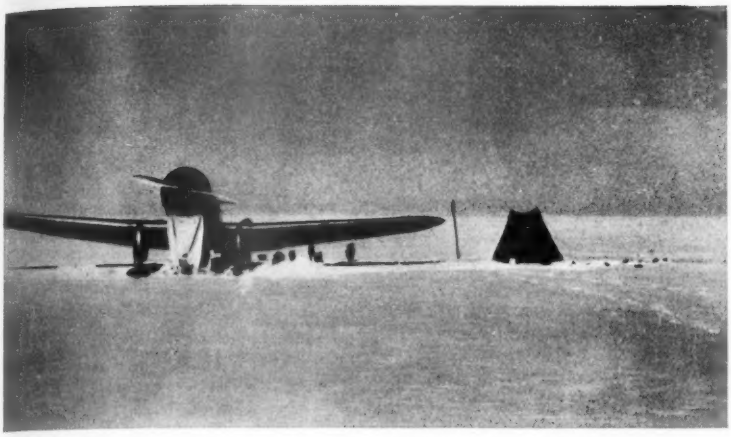
On December 11 we travelled 10 or 11 miles, sledging by low sun, and had one bad pull over a crevasse. It was weary work, and it seemed as though we must be going in the wrong direction, for the never-ending expanse stretched on for ever. The low night sun cast a dull glow over the ice fields without warmth, although the sky was cloudless. Suddenly I told Kenyon I could see a line of blue water on the horizon. It was the Bay of Whales, and we had been travelling much too far west.

On December 12 however, although we marched 12 miles, we were unable to find the water which we had seen the day before. On December 13, travelling entirely by compass as before, in misty weather with snow flurries, we made another 10 miles. We approached a ridge and hoped to get an extended view. Topping it we looked straight down into salt water. We had heard the lapping of the waves and thought it was the wind, but it really was the sea at last.

On December 14 we reconnoitred and in the evening took a sight, to find we had travelled about 10 miles too far north, and must go back south. We judged that we were at the mouth of the Bay of Whales. On December 15 we travelled 15 miles and came, at "Ver-sur-mer," Byrd's unloading place, upon two tractors half-buried in snow. This gave us our position, so we dragged on up the east side of the bay, topped a rise, and looked down upon the most desolate remains of past habitation that I have ever witnessed: only a lot of masts and the stove-pipes of buildings sticking out of the snow. We broke through a glass skylight, and were able to let ourselves down into what proved to be the radio shack.

On December 16 we dug a tunnel and made steps down to the door of our shack. We found coal, gasoline, and some welcome stores. We cleaned up everything, and settled down to a routine to await the arrival of the *Wyatt Earp*. Every day I walked 6 miles down to the tractors where we had put up our tent, with two yellow streamers and a note that we were at Little America, so that the *Wyatt Earp* could know where we were.

On January 15, a month after we arrived at Little America, I was awakened



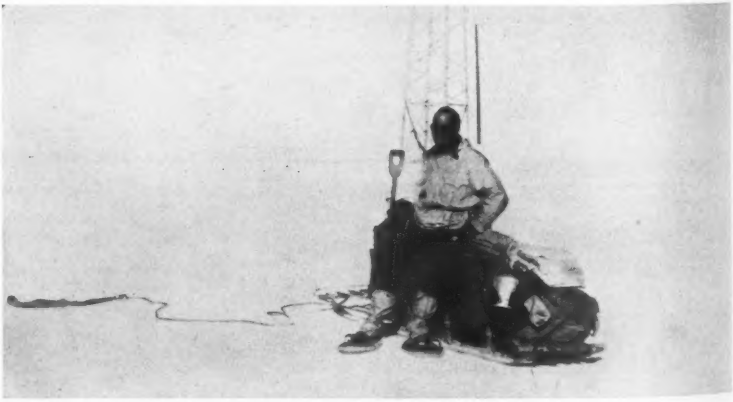
45. 24 November 1935. 17.29. The aeroplane in Camp II

B. The aeroplane snowed up in Camp III

53. Sledge packed for departure from Camp V [December 9]

51. 9 December 1935. Start for Little America

62. Lincoln Ellsworth on arrival at Little America. December 15



64. Hollick-Kenyon at Little America

at 22.00 to see Kenyon standing over me with a note in his hand. He had heard the roar of a motor overhead, although our dug-out home was 15 feet beneath the snow, and had crawled up to the surface in time to see a parachute descending through the fog which had enveloped us for two weeks. The parcel contained food, and the note was from Captain Hill, commanding the R.R.S. *Discovery II*. Within ten days after the failure of our radio the Commonwealth of Australia had sent a relief expedition, and had been seconded in this by the Governments of the United Kingdom and New Zealand. As I was laid up with an infected foot, Kenyon started off alone to meet our visitors; but I could sleep no more that night, and started out in snow-shoes to learn what was up. A mile from camp I saw through the fog, which magnified frightfully in these regions, what appeared to be a whole army of men marching towards me; in reality there were six of them. We packed the sledge and started for the ship, where I was received with open arms, and learned that my own ship had been delayed by the pack-ice in the Ross Sea. Three days later a radio message told us that the *Wyatt Earp* was approaching the bay, and very soon the staunch little craft loomed up in the fog. While my party was loading the *Polar Star* on the *Wyatt Earp* I went on the *Discovery II* to Australia, where I was for twelve days the guest of the Government.

APPENDIX I: NOTE ON THE NAVIGATION

The machine was fitted with a Pioneer drift indicator, but it was found difficult to measure drift over featureless snow. We had no means to measure the ground speed directly, and in fact it proved to be much slower than we had calculated from the known air speed of the machine and our estimates of the wind.

We had based our plans upon an average ground speed of 145 to 150 m.p.h., derived from tests of the plane with skis made at Winnipeg, though not, it is true, with the full load. We soon found on the flight that the air speed must be less than anticipated, for there was no sufficient evidence of wind to account for the reduction of the ground speed to about 120 miles on the average. We have never been able to account for this loss of speed.

In planning our navigation we had to remember that the tail of the plane hid the sun when it was within about 15 degrees of the course astern. We had chosen our starting hour so that the sun should be abeam in the middle of the flight, which should have allowed us to determine by sights that we were on our course, though not precisely where upon it; but our loss of speed would rather have defeated this intention even if the sextant had not gone out of adjustment, and in fact our observations for position were, in the end, all taken from the ground except one position line at 16h.

Our scheduled route plotted upon the map of the Antarctic in four sheets, published by the American Geographical Society of New York, provided for fourteen and a half hours' flying at 145 m.p.h. over the ground. For the end of each hour we had worked out the true bearing of the Great Circle course, which ranged from 202° at the beginning to 304° at the end, changing by two or three degrees per hour in the first half and then more rapidly. To this we applied the compass variation, which changed from 14° east at the start to 105° east at the finish, its total change and its rate of change being curiously similar to the change in the true course but of opposite sign. In consequence the magnetic course varied very little, being constant over the first half at 188° or 187°

and then varying irregularly up to 199°. It was due to this happy accident of the compass variation balancing the change in the true bearing of the Great Circle course that we kept very close to our course, although on the first half of the flight we were far behind our scheduled time. Instead of having to change course by a number of degrees per hour we were able to fly upon a fixed compass bearing for fourteen hours, and were relatively little off our course at the end of that time.

We carried a Bausch and Lomb bubble sextant of Bureau of Standards Type C; a high-grade watch by Patek-Philippe; an American *Nautical Almanac* with the new table of Greenwich Hour Angle, and Ageton's tables (Hydrographic Office No. 211) with blank computing forms adapted to these tables. In preparation for the flight begun on November 21 we had computed curves for the altitude of the sun at scheduled points in order to be able to solve the position-line problem by inspection. It is worth while to work many hours in comfortable quarters if it saves only a few minutes of work in the air.

We were however compelled to turn back on November 21. We did this computation all over again for November 23, but little use was actually made of these prepared altitude curves, because first of the accident to the sextant, and secondly of the long delay in camps en route.

The bubble of the air sextant is set in adjustment by a lock-nut. It seems that this must have worked loose between the first and second sights in the air on November 23, for the results worked from these two observations were impossible and, similarly, the sights obtained when we descended at Camp I had no reasonable solution. Our long delay at Camp II enabled us to take a great number of sights, and eventually to determine that the bubble, after having slipped so that the index error was about 82′, had since remained constant, and when this was determined we were able to work back and reconcile the sights made on November 23.

We used a 7-inch Aperiodic magnetic compass (Pioneer Company), and it gave excellent performance. We made no attempt to compensate for all deviations since over-compensation makes the compass sluggish in the neighbourhood of the Magnetic Pole.

APPENDIX II: EQUIPMENT

At our take-off from Dundee Island the loaded plane weighed 7987 lbs. made up as follows:

Plane (empty)	3614 lbs.
466 gallons of gasoline	2796
Oil	160
Food	260
Sledge with cover and lashing	98
Primus stove, skis and tools	80
Fuel for stove	18
Sleeping-bags with wooden slats for protection against wet snow	32
Cameras	10
Sledge radio set	50
Miscellaneous and spares	449
Pilot and navigator with equipment	420
	7987

The food was principally pemmican, sunwheat biscuits, bouillon cubes, bacon, butter, milk powder, dried fruit and tea, and the full daily ration for 61 days was 34 oz. computed to contain 15 per cent. protein, 28 per cent. fat, 56 per cent. carbohydrates, giving 4860 calories.

The pemmican was of two brands—the Amundsen type made by the Beauvais Co. of Copenhagen, and another made to the formula of Mr. D. C. Coman, of Johns Hopkins University. The latter had some pepper in it and for my part I preferred it. I always find that one or two fixed rations are better than a great variety. On this flight we had for breakfast a mug of oat porridge with cubes of bacon; dried milk and sugar, and oat wafers with butter. For supper, a mug of pemmican with biscuits and butter. I never tired of this and always looked forward to it. We must have used much less than the 34 oz. per day of our schedule.

There were two radio sets. The main set transmitted on wave lengths of 20 to 80 metres and on 600 metres. The transmitter with 100 watts output was worked by a generator coupled directly to the engine; the antenna was of the trailing-wire pattern. Until it broke down this set transmitted successfully to the *Wyatt Earp*. To energize the transmitter when the plane was not in flight we had a portable petrol engine generator of 300 watts.

The sledging radio set could transmit and receive over a wave length of 30 to 100 metres. For transmitting it was operated with a 15-watt generator, turned by hand or driven by a foot treadle. It seemed unreasonable to suppose that all these means should fail, but in fact we did fail to keep in touch with either the ship or civilization.

Radio equipment should be mounted so that it can be easily dismounted and taken apart, and nothing should be soldered which can be secured by other methods. Our principal radio set broke down apparently by a connection shaking loose, but the fault was never quite established, for the set was not properly accessible.

I myself wore during the flight camel-hair underclothing and socks, fleece-lined socks, moose-skin moccasins, woollen breeches, squirrel-skin parka, reindeer-skin parka, chamois face mask, silk gloves, woollen gloves, dog-skin mittens, woollen cap, and snow goggles.

Hollick-Kenyon's outfit varied from this by an additional pair of socks, trousers instead of breeches. Canvas rubber-soled boots with insoles instead of moccasins, only one parka, an additional leather flying cap with earphones, and another pair of woollen gloves instead of silk.

Note on the construction of the map, by the Editor G.J.

The Great Circle distance from Snow Hill Island to Little America was calculated at 1842 nautical miles, and "stations" numbered from 1 to 14 were plotted on the American Geographical Society's map at distances of 130 nautical or 150 statute miles from this origin, or one hour apart at the anticipated speed. In fact the flight was started from Dundee Island, 66 nautical miles back, so that stations 1, 2, 3 . . . should have been reached after $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$ . . . hours' flying. These computed stations are shown upon the Great Circle course plotted on our map.

The A.G.S. map shows also, mostly in dotted outline, as imperfectly known, the east coast of Graham Land as far as Cape Eielson, presumably from the results of Wilkins' flight. So far the pilot, Hollick-Kenyon, had some material, though rough, to check his ground speed; and from the notes in his log it is possible to indicate roughly the times along the earlier part of the flight. The

actual trace has been taken from a plot, by Commander Weems, from the pilot's log.

The longitude of Cape Eielson is very uncertain. On the A.G.S. map it is 62° 35′ W. On the preliminary sketch-map published in the *Geographical Review* for August 1936 the cape is moved west to 64° 20′, and the strait or fjord observed by Wilkins and confirmed by Ellsworth is shown stretching well away to the west of the cape. But this brings it across what is believed to be the position of the unbroken west coast of Graham Land traversed by Stephenson, Bertram, and Fleming in the autumn of 1936, to a point about 90 miles farther south than the point where Wilkins turned (*v.* wireless despatch from the British Graham Land Expedition, *The Times*, 12 December 1936). We have therefore left Cape Eielson in the longitude shown on the A.G.S. map, have abbreviated the length of "Stefansson Strait" there shown, and have indicated the unbroken west coast and the long strait between it and Alexander I Land by dotted lines. The stretch of the west coast farther north between lat. 66° 30′ and 64° 20′ has been plotted from the charts sent home last summer from the first year's surveys of the B.G.L. Expedition.

The new mountain ranges discovered and photographed by Mr. Ellsworth have been indicated conventionally in their approximate positions from the log, but no attempt has been made to derive from the notes in the logs or from the photographs their bearing and extent. We understand that the material is now being worked up by the American Geographical Society. The estimated heights of the ground, and of the principal peaks of the new mountains are taken from the logs.

If the adopted latitude of Cape Eielson is correct the ground speed during the first few hours of the flight must have been about 125 miles per hour, and thenceforward have averaged less than 100, as far as Camp I. The times marked along the track are the log times of passing land marks, and when these were finished, the position roughly interpolated for each hour, to indicate how much they were behind their dead reckoning.

The astronomical positions of the five camps shown upon the map are from the final calculations communicated by Commander Weems. They may be compared with the discordant results obtained before the large index error of the sextant, that developed early in the flight, had been happily discovered and corrected at Camp III, thus remedying a situation which must at one time have seemed desperate.

Camp I. Dead reckoning gave	80° 28′ S.	141° 02′ W.	
Sights: 1st set gave	80 20	104	
2nd set gave	80 43	85	50
Final corrected calculation	79 15	102	35
Camp II Sights: 1st set gave	80 58	90	00
2nd set gave	77 42	117	30
Final corrected calculation	79 22	107	30

Careful arrangements were made to cover the possibility of engine failure and forced landing in a position from which no further progress could be made by flight, while wireless communication had failed. The route was divided into sections, which are shown on the map by lettered arrows: and for a landing in each section the following procedure was laid down:

Section A: Follow the Larsen Ice Barrier to Robinson and Robertson Islands, through Crown Prince Gustav Channel to rendezvous at Hope Bay, looking for depots and leaving notes on S.W. corner Robinson I., N.E. corner Robertson I., and Cape Longing.

Section B: Make for C. Pierre Baudin, Marguerite Bay, through Bourgeois and Lallemand Fjords to Matha Bay, C. Evensen, and rendezvous at Pitt Island, or if that is inaccessible S.W. corner of Beascochea Bay, leaving notes and looking for depots on C. Pierre Baudin, north tip of cape opposite Laird I., and Cape Evensen.

Section C: Make for Mt. Martha and depot on S.E. corner Charcot I.

Section D: Make for N. edge of landfast ice and eastward to Mt. Monique, Charcot I., looking for depot on S.W. corner.

Section E: Make for Mt. Mabelle Sidley, look for depot, and wait for relief; or leave note there and proceed to Mt. Grace McKinley.

Section F: Make for Little America.

Provision had been made for obtaining a relief aeroplane to reach some of these positions, and a system of ground signals by which to communicate with this machine.

The calculations of courses and sights, with copies of logs, and much other material, were deposited with us by Commander Weems, U.S.N. ret., who had been interested in developing the methods and had undertaken to discuss the results for Mr. Lincoln Ellsworth. He returned to the United States in October last. The presentation of this material and the construction of the provisional map were discussed with Mr. Ellsworth during his brief visit for the lecture on November 30, and by his permission the paper is now published without the delay which would have been incurred if proofs of text and map had been submitted to him for revision. Should there be, and there probably must be, some mistakes of interpretation, the responsibility must fall upon the Editor of the *Journal*.

Note on the photographs

The numbers and times (in Greenwich Civil Time) on the prints kindly sent us by Dr. Joerg at the request of Mr. Ellsworth correspond with the list of photographs left us with other material by Commander Weems. This list gives some brief indications of subject, but often only whether to right or left of the course. It has seemed better to preserve the original numbers than to renumber serially those which have been chosen for reproduction. The pictures were taken with a Leica camera, operated with some difficulty from a machine not very convenient for photography, so that the horizon is often tilted. The blocks have been squared to the horizon by estimation, and the interpretation of Nos. 9 and 11 may be thereby affected: whether the ice in the middle distance on the left is to be taken as sea-ice or a glacier descending gently depends very much on the estimate of the horizontal. The picture lettered A is not dated, but must, we think, belong to the flight of November 21, to which No. 6 also belongs.

No. 7 should be compared with the picture numbered 6 in *Geographical Review* (July 1936) taken 2 minutes later; our No. 21 with *G.R.* 10a taken 24 minutes earlier. Our No. 6 is *G.R.* 8a; 11 is *G.R.* 2; 23 is *G.R.* 11b, and 24 is 11a. Our No. 26 is *G.R.* 12, and 39 is *G.R.* 17. The diagram constructed by Dr. Joerg to give a preliminary sketch of the results puts Cape Eielson farther west, as noted above, and one must bear this in mind when comparing our tentative plot with his.

There remain a few slight discrepancies. The date and time assigned to No. 45 does not fit the narrative and the Camp number is uncertain. The date assigned to No. 53 is December 4–5, but December 9 would suit the narrative. The plate lettered B is not dated, and the Camp number is presumed.

DISCUSSION

Before the paper the PRESIDENT (Professor HENRY BALFOUR) said: We are about to spend a very interesting evening. Mr. Lincoln Ellsworth is probably known to you all as an airman of great distinction, one of the most enterprising, in fact, with a long record at his back. He has flown extensively in both the Arctic and the Antarctic regions. His *penchant* appears to be for the colder climes. He has, I believe, made at least three expeditions into the north polar area and three into the Antarctic region, which shows considerable impartiality on his part. In the course of his flying journeys Mr. Lincoln Ellsworth has come into contact with many remarkable people, of whom I may perhaps mention Roald Amundsen, that great Norwegian explorer, with whom he co-operated on more than one occasion.

I am not going to attempt to tell you all that Mr. Lincoln Ellsworth has done, because I know you are looking forward to his own account of his more recent adventure. You would, I am sure, sooner hear him tell of it than listen to me giving a second-hand version, and so without further preamble I ask Mr. Ellsworth to give his lecture upon his recent flight over the Antarctic region.

Mr. Lincoln Ellsworth then read the paper printed above, and a discussion followed.

Mr. S. G. MCFARLANE: I would like in the first place to apologize for the absence of the High Commissioner for Australia. Unfortunately he is indisposed, otherwise he would have been very pleased to be present this evening.

It has been a great privilege to listen to Mr. Lincoln Ellsworth's epic story of one of the most interesting polar expeditions that have, I think, ever been undertaken. I have been completely absorbed, as I am sure we all have been, by the account of the novel and courageous adventure which Mr. Lincoln Ellsworth has presented in such a fascinating manner to-night. I remember well that Sunday afternoon in Canberra when the request came through to Mr. Lyons, the Prime Minister, that an expedition should be organized to go to the rescue of Mr. Lincoln Ellsworth. As you all know, that expedition was promptly sent. We have seen to-night that Mr. Ellsworth was perfectly safe all the time; that he had almost reached his destination when his wireless ceased to function, and that the *Discovery II* arrived in the Bay of Whales just two days before the *Wyatt Earp*, his own vessel, got there.

I wish to congratulate Mr. Ellsworth on his great adventure and thank him for his most fascinating and interesting lecture, which I am sure we have all enjoyed very much indeed.

Dr. H. R. MILL: I have been puzzled for the last hour to know whether it was the modesty or the humour of this lecture which made it so attractive, but I have had to give it up. I suspect that it was the mixture of both. I do not think I have ever heard a lecture delivered more quietly and with less sensationalism, or with more real feeling of the tremendous adventure which was being described.

We have now been convinced that the instrument for future polar exploration is the aeroplane; at least I have been so convinced. But the aeroplane must not stay in the air for all the short time necessary to carry out its journey. The expedition we have heard of to-night is a pioneer expedition, which ranks as regards the principle involved in the same category as Nansen's first crossing of Greenland, when, for the first time, he solved the problem of the line of retreat. In the old days the question always was: "How are you going to get back?" Nansen said, "I am not going to get back at all. I am going right through." Now Mr. Ellsworth has carried the same principle into the air. When Wilkins

made his fine flight along the coast of Graham Land he had to travel only half the distance that his fuel could carry him, because he had to get back again. When Byrd made his great flights from Little America he too had to reserve supplies for his return. But when Ellsworth started from Dundee Island he planned to carry on for the whole distance his petrol would take him, because he was going right through from the less accessible to the more accessible end of his journey. That is the great principle of *Forward* which Nansen laid down nearly fifty years ago and which we have now seen transferred from the land and sea to the air.

Mr. Ellsworth very rightly said that a great deal more remains to be done. He drew that sure line across the unknown, fixing his position accurately at several points by astronomical observations. Now we want to know how far it is from that line to the coast, how far the plateau extends towards the Pole, whether it rises continuously or whether there are valleys or mountains in the way. In other words, we require many stations established whence aeroplane reconnaissance and journeys on foot can be made in all directions.

There is one point, a sort of moral, that I would like to stress. When I first heard papers on the Arctic, far back in the time when Greely was returning from his great expedition, the front row at the meetings of this Society was always occupied by a magnificent line of ancient admirals. There were many who had been trained in the Arctic regions, back to the time of Admiral Sir Erasmus Ommanney, who fought at the battle of Navarino in the year 1827, and commanded a ship in the Franklin search. There were McClintock, Vesey Hamilton, Inglefield, Nares, Albert Markham, and many more, all of whom were trained for their great careers in the Navy by their experience in the Arctic regions, as Nansen himself had been. They were taught resourcefulness, the power of making quick decisions, of taking advantage of sudden opportunities and of meeting unexpected emergencies. The Navy under such men has done a great deal in the way of exploration in the past, but the future of discovery is in the air. I sincerely hope that our Government will take the opportunity of training young air officers by sending them into the polar regions on geographical expeditions. There they will learn the same lessons of resourcefulness that the naval officers learnt before and to a higher degree. Perhaps at some future period, long after my time, the front row here will be occupied by a fair array of venerable air marshals who will go back in memory to the grand old days when they conquered the last unexplored portion of the Earth, and who will look on Mr. Ellsworth as the great pioneer who showed them how to change the half-and-half method into that grand spirit of going always forward and right through.

I have to thank you, Mr. Ellsworth, for giving me one of the most delightful evenings I have ever had in this Society.

Mr. J. M. Wordie: Mr. Ellsworth has mentioned the prompt action of the Discovery Committee in diverting *Discovery II* from her research programme south of Australia to go to the Ross Sea when it was known that Mr. Ellsworth and Mr. Hollick-Kenyon were missing. The Australian and the New Zealand Governments were faced by a difficult situation; in the circumstances they had a moral responsibility, and to delay meant losing the short navigable season in which rescue was possible. The *Discovery II*, as it happened, was at work in the neighbourhood, and it seemed obvious that she should at least stand by in case of emergency. The initiative came from the Commonwealth Government of Australia, at whose suggestion the Discovery Committee was approached on behalf of the United Kingdom, Commonwealth, and New Zealand Governments, and in its turn gave orders, and within twenty-four hours the *Discovery II*

was on her way to Melbourne to refuel to go down south and stand by in the Bay of Whales. As events turned out, her voyage south was unnecessary, but I think you will agree with me that those in Australia and those in New Zealand responsible for the Ross Dependency, and finally those at home at the Colonial Office, adopted the only possible course of making sure that Mr. Ellsworth and his companion were safe and secure. You all know how quickly and efficiently the rescue took place. At the same time, this effort has served, I hope, to further the good feeling and the mutual respect which prevail between British and American explorers in the Antarctic.

Ellsworth himself has added more to the map of Antarctica than almost any other man. I cannot say "than any other man" without qualification, because one cannot properly compare the work of different individuals. In my own mind however I liken him to Amundsen for the reason that Amundsen, before his expedition, carefully thought out Antarctic conditions: whether it would be possible to work with dog-teams, and especially whether it would be possible to place his base on the Great Ross Barrier. This was against the advice of all those who had been there before him, but events proved Amundsen right. In the same way, Ellsworth, aware that landing an aeroplane on snow and ice is always a difficult matter, and that it is equally difficult to be sure that the plane will take off again, carefully analyzed snow surfaces from books of former travel, and decided in his own mind that he could be almost certain of landing at will and that a flight across the Antarctic could therefore be carried through successfully. His judgment and foresight link him, in my mind, with Amundsen, and in a very fitting way, as it was Amundsen himself who initiated Ellsworth into Arctic flying away back in 1925 and again in the year following, when they made their joint voyage across the North Pole in the *Norge*.

By his discoveries Ellsworth has solved an Antarctic problem, but at the same time his flight has drawn attention to a great many more. We are faced with a new Antarctic plateau at a lower level; but we still await the solution of what happens to the mountains of Graham Land. These Graham Land mountains are true fold ranges, and are quite distinct from the scarp mountains of Victoria Land; and we know that the two cannot fuse into each other. We cannot make two different geological structures meet end on. The mountain lines of Graham Land must either lie up against Victoria Land or else sheer away long before they reach the Ross Sea. Since Ellsworth's flight it now looks as if the Graham Land mountains do not run across the Antarctic to the east side of the Ross Sea, as formerly thought, but as if they more probably shoot out into the Pacific not far south of Graham Land; the structure may in fact be very much like the western side of Europe, with the Pyrenees ending off abruptly against the Atlantic. This may also be the case in the Antarctic; there is much still to learn, and Mr. Ellsworth by his great flight has certainly set us further problems for solution.

Prof. DEBENHAM: I am very grateful to you, Mr. President, for having given me this opportunity of expressing my admiration of Mr. Ellsworth and of his flight. I do not know anything about Mr. Ellsworth's ancestry, but there is, I am sure, one name that must figure in it—that of Robert Bruce. You will agree with me that it must need a tremendous amount of cold courage to plan such a flight, but to try and try again, three times, needs more than most of us can claim: the first time after having his plane wrecked; the second time after spending months and months in what is rather a miserable climate waiting for flying weather.

I think Mr. Wordie has pointed out the most significant point in connection with this amazing flight. It was great in distance, but other flights have also been great in distance. The significant thing was the landings. I am afraid

that until I read of Mr. Ellsworth's expedition I was old-fashioned enough to believe and to say that the aeroplane, excellent as it may be for discovery, was not an instrument of exploration. There is a clear distinction between those two terms, because although you can discover from the air you cannot really explore the detail from above. I have been waiting for the time to come when airmen would take off and land almost at will, and it is approaching.

What impressed me most in this lecture by Mr. Ellsworth was the calm way in which, when he was 1000 miles from anywhere, he said, "We were not quite sure of our position so we thought we would land to find out." When there has been a flight of something like 1000 miles into the unknown with only two men on the flight, for one to put his head out and decide to land to find the position is what I have been waiting for, and it shows more courage than the flight itself. Mr. Ellsworth and Dr. Mill both pointed out what that may lead to. There were no dogs on board this plane, but as soon as an aeroplane can land when it desires and can be fastened down so that no accident happens to it, then will be the chance for the aeroplane to become an exploring instrument, because it will take dogs and men over the difficult part, and from there they can start off on a sledge journey. The most valuable part of Mr. Ellsworth's amazing flight was the repeated landings he and his companion made, and I think most of us would have preferred to go on to within a few hundred miles of our goal before we decided to land and ascertain the position.

The PRESIDENT: The evening has been spent in an exceedingly interesting manner. We have listened to the story of a great enterprise planned and carried out in the most admirable and determined manner. It is apparent to us all that it was a great flight.

The speeches to which we have listened have practically exhausted the subject, and have called attention to the real merits of the enterprise. I need not therefore keep you, but I would like to mention that Mr. Ellsworth has come across from the United States on purpose to give his lecture to us to-night. Unfortunately, he is only spending a very few days in England. I wish that he could be here longer so that we could hear him still further. At any rate, it is gratifying to us to realize that he has thought it worth while to come all this way—not that distance can mean very much to him—in order to give us an extraordinarily informative and entertaining lecture. I ask you to express your thanks to him both for the wonderful work which he has done, for the light he has thrown upon the vast continent of the Antarctic, and also for his lecture.

CLIMATIC CYCLES: *A paper read at the Afternoon Meeting of the Society on 18 January 1937, by*

PROFESSOR D. BRUNT, M.A., B.SC.

IT is well to start by defining accurately what we mean by a cycle. The Oxford English Dictionary gives a number of definitions, the one which is applicable to our subject reading as follows: "A period in which a certain round of events or phenomena is completed, recurring in the same order in succeeding periods of the same length." Such a definition excludes the possibility of applying the term "cycle" in such a way as is done for example by Huntington

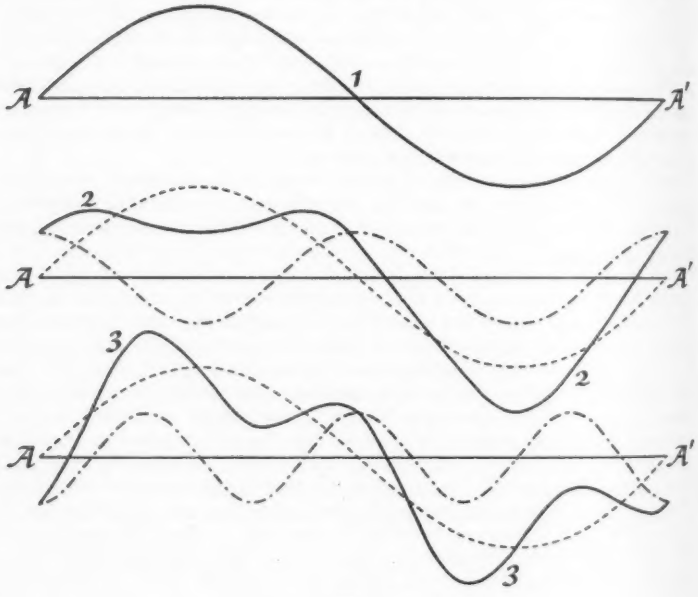


Fig. 1

in his paper on "Climatic Pulsations."[1] In this paper Huntington describes as a cycle the interval between two successive peaks in a curve of temperatures, when there is no evidence of anything that can be described as a recurrence of what he calls a cycle. I do not regard it as useful to allow the name cycle to be applied to the interval between any pair of successive peaks in a record of temperature, rainfall, or any other meteorological phenomenon. That temperature and rainfall show wide swings, when recorded over a long interval of time, is sufficiently well known to need no discussion. The diagrams shown later, in Figs. 2, 3, 4, 5, and 6, show examples of such swings. If we

[1] *Geografiska Annaler*, 1935.

restrict the use of the word "cycle" to denote regular variations, it becomes desirable to adopt some word as a name for the irregular variations such as are shown in Figs. 2, 3, and 4. Simon Newcomb, in discussing variations of much the same character in the moon's longitude, used the name "fluctuations." We might well follow Newcomb in this, and so avoid debasing the word "cycle" and the confusion which now arises on account of the lack of uniformity in the meaning ascribed to it.

2. *The Harmonic Cycle*

Most of the research which has been carried out into the existence of cycles in weather has been based on the assumption that the cycles in which nature works are harmonic cycles, or sine-curves, such as the one shown in curve 1, Fig. 1. Curve 2 in Fig. 1 is a combination of curve 1 with a cycle of half the period and half the amplitude of curve 1, while curve 3 is a combination of curve 1 with a cycle of one-third the period and half the amplitude of curve 1. The peaks in all these curves are rounded, so that if the length of the period of the wave is thirty years, we should expect the peak effect to cover several years.

The use of the harmonic curve as a basis of argument has several advantages, the most obvious of which is that the mathematics involved follows a clearly defined method; and a second is that there exist certain criteria by which we can judge the value of the periods found, in interpreting the observed variations of meteorological phenomena. The weakness of the use of the harmonic cycle lies in its assumption that nature works essentially in sine-curves, an assumption which is legitimate in certain types of dynamical problems, but which is not of necessity so in other types of problems. There are phenomena which follow other well-defined types of curves. For example, on a clear still night the temperature trace from sunset to sunrise approximates very closely to a parabola, and the recognition of this fact is more useful than any analysis of the curve into a number of harmonic variations.

3. *Some observations of temperature and rainfall*

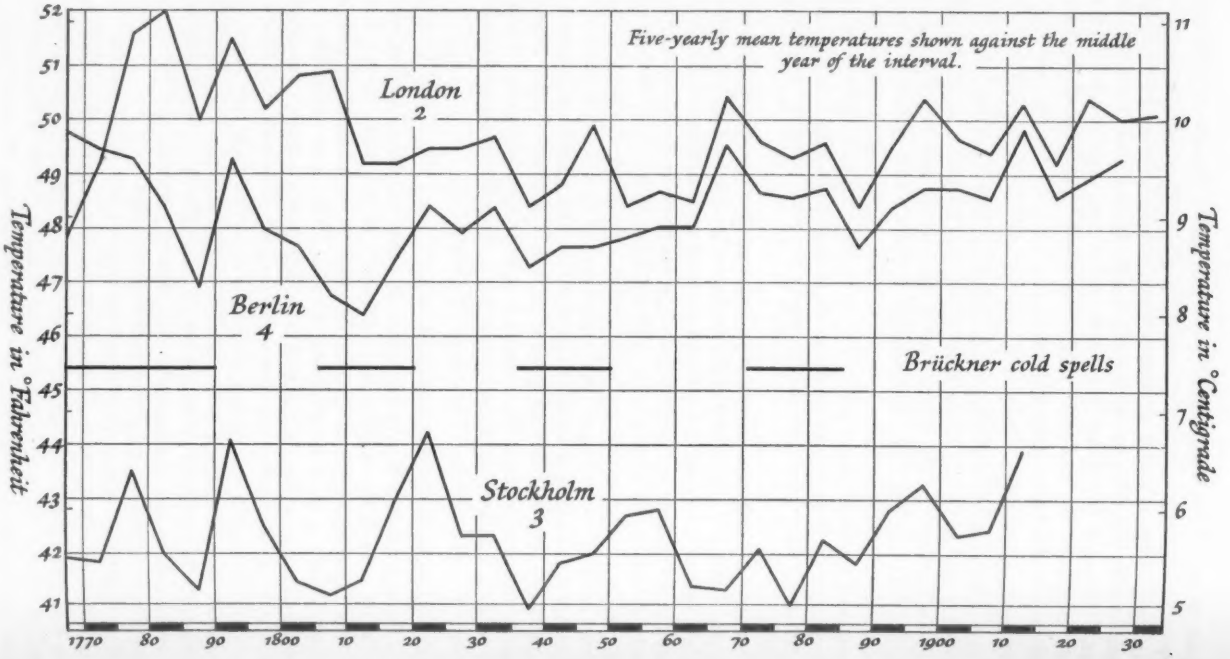
In Fig. 2 are shown five-yearly means of temperature in London, shown against the date of the middle year. The form of the curve does not suggest that the temperature of London is likely to be represented with any accuracy by any cycle, or combination of cycles. The curve shows very wide variations of temperature, the rise within the first twenty years or so amounting to over 4° F. It may perhaps be urged that the early observations of temperature are doubtful, but we can only accept the observations as they stand, particularly as they are to some extent confirmed by the general descriptions of weather and the statistics of skating which are available. The general trend of London temperatures as shown in Fig. 2 can be described as:

(*a*) A very rapid rise from about 1760 to about 1780;

(*b*) A rapid fall from about 1780 to about 1835; and

(*c*) A slow recovery, with some interruptions, from 1835 to the present day.

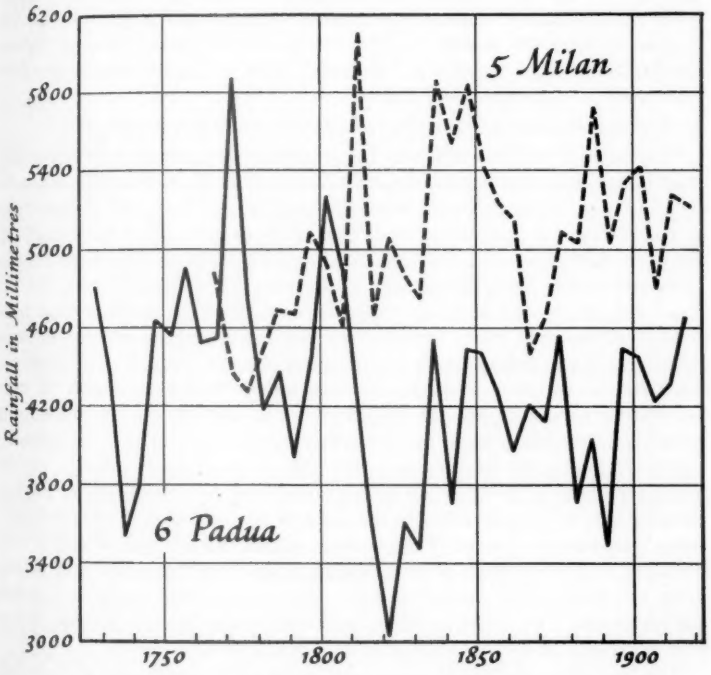
In Fig. 3 are shown five-yearly means of temperature in Stockholm. At first sight this bears very little resemblance to Fig. 2. It shows a succession of roughly wave-shaped variations, with varying intervals of time between the successive peaks or dips in the curve. A closer examination shows that the


Five-yearly mean temperatures shown against the middle year of the interval.
London
2
Berlin
4
Stockholm
3
Brückner cold spells
Temperature in °Fahrenheit
Temperature in °Centigrade
52
51
50
49
48
47
46
45
44
43
42
41
11
10
9
8
7
6
5
1770
80
90
1800
10
20
30
40
50
60
70
80
90
1900
10
20
30

minima in Fig. 3 are all represented in Fig. 2, though on a smaller scale. The peaks in Fig. 3 are not equally faithfully reproduced in Fig. 2.

In Fig. 4 are shown five-yearly means of temperature in Berlin. A number of the peaks and dips in Fig. 4 coincide with those in Fig. 3, but the general form of Fig. 4 shows a closer resemblance to Fig. 2 than to Fig. 3. Still earlier Berlin temperatures, which are perhaps not very reliable, show that about 1740 the curve should come to a lower level than even in 1812.

A curve of five-yearly mean temperatures in Edinburgh differs from Fig. 3 only in the absence of the dip at 1787, the extent of the variation of temperature at Edinburgh being about half that of the Stockholm curve. A similar



curve of Paris temperatures differs from the Berlin curve of Fig. 4 in having its lowest dip at 1787, and in having the peak at 1792 and the dip at 1812 shown as only slight interruptions in an upward trend of temperatures from 1787 to 1827. The curve of Vienna temperatures shows dips at 1787 and 1812, and a peak at 1792, but far less prominently than does the Berlin curve of Fig. 4. The lowest dip in the Vienna curve occurred from about 1837 to about 1852.

Thus an examination of the actual observations of temperature at a number of European stations shows that wide variations of temperature have taken place from time to time. The years around 1772, 1787, 1807, 1837, and 1887 were years of relatively low temperature, while the years around 1777,

1792, 1822, 1847–1857, 1867, and 1897 were years of relatively high temperature, though the variations at these epochs were not equally effective at all six of the places mentioned above. In other words, there were active from time to time some physical causes of variations of temperature which did not affect the same area of Western and Central Europe at each epoch. It must be noted also that the differences between the temperature variations at various places of observation are to be explained not as differences in the time of incidence of these swings of temperature, but as differences in their effectiveness at different places.

Rainfall shows even greater variability than temperature. Figs. 5 and 6 show the variations in the five-yearly totals of rainfall at Milan and Padua. The resemblance between these two curves is meagre, and it would be difficult to give any definite points of close resemblance between the two curves. Similar curves for London and Edinburgh show very little resemblance with each other, or with Milan and Padua.

4. *Practical evaluation of the effect of a period of any assumed length*

The method used in evaluating the amplitude of a period is similar to that used in obtaining the form of the annual variation. If, for example, we had a series of monthly mean temperatures for 100 years, we should arrange these in 100 rows of 12, one row for each year, and one column for each month of the year. Adding the columns and dividing by 100, we should obtain a series of figures representing the average yearly variation of temperature. Having done this, we should in practice find it worth while to subtract from each individual monthly mean temperature the hundred-year average for that month, so as to represent each month in the interval covered by the observations by the variation of its mean temperature from the normal. If now we wished to investigate the amplitude of a possible period of fourteen months, we should arrange the deviations from normal in rows of fourteen, finally averaging the columns as in the case of the annual variation, and so getting the form of the variation having a period of fourteen months. For the investigation of periods of length ten years or more we should use the annual mean temperatures instead of the monthly values. An example of a set of data obtained in the first stage of investigating a period is shown in Fig. 7, which gives the form of the curve obtained for twenty-three years in London temperatures. The curve joining successive points is not smooth, but a sine-curve can be drawn to fit the points fairly well.

The procedure described in the last paragraph is quite simple, provided we know what length of period we want to investigate. The real difficulty is to decide what periods should be so treated. In any investigation of cycles which starts from the beginning, with no preconceived idea as to the periods which may occur, it is necessary to carry out the computation of the amplitude for a wide variety of periods. The number of possible periods to be found in, say, one hundred years of monthly mean temperatures is extremely large, but fortunately it is not necessary to try out every possible value. It is sufficient to work out the amplitudes of periods separated by unit difference of frequency in the interval covered by the whole series of observations. If all the trial periods prescribed by this rule are investigated, we may feel confident that no large amplitude will have been completely overlooked.

In some investigations of periods in London temperatures and other records, which I carried out some years ago,[1] I attempted to investigate all possible periods from about one year up to about thirty-five years. In Fig. 8 are plotted the amplitudes of the trial periods evaluated, the horizontal scale being the reciprocal of the period, or the frequency. Such a diagram is called a "periodogram," following Schuster. Considerable work was also done by other methods than direct computation to ensure that no intermediate period having a large amplitude was overlooked, and so the diagram may be taken

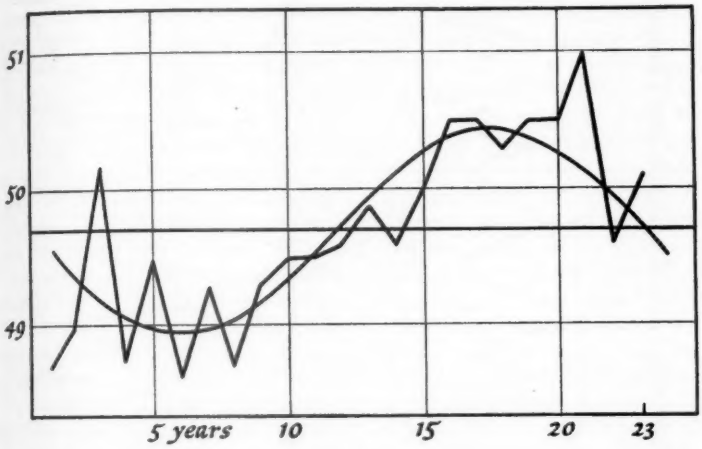


Fig. 7

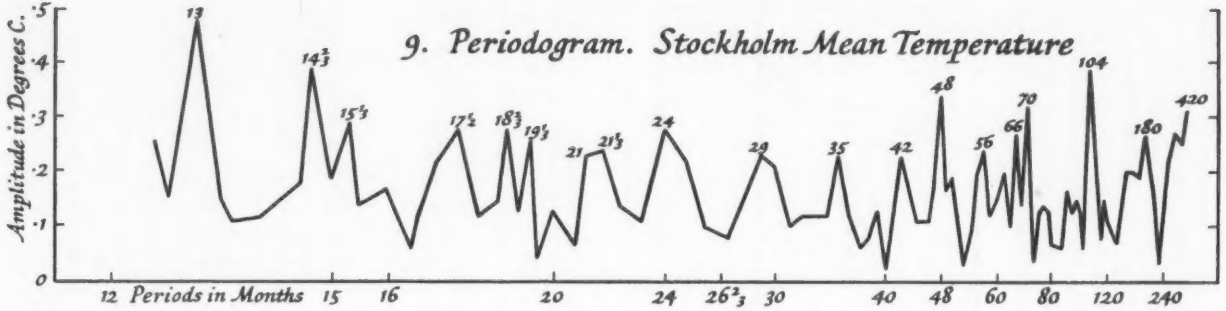
as giving a fairly faithful picture of the amplitudes yielded by the London temperatures. Fig. 9 shows the result of a similar investigation of the Stockholm temperatures.

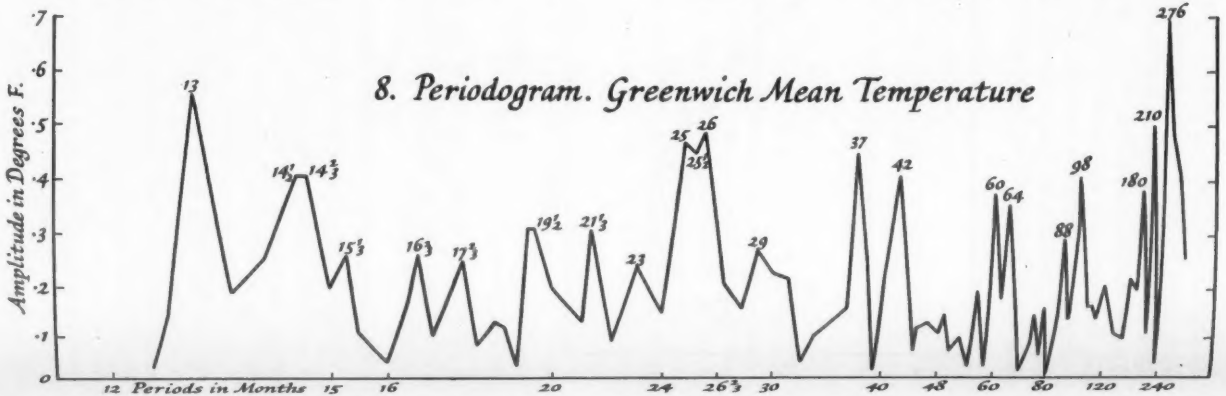
5. *The interpretation of the results obtained*

The aim of all statistical treatment of arithmetical data is to find a method of representing the results which shall be simpler than merely giving the original aggregation of observations. If the individual observations are repetitions of some measurement in which the time between the repetitions is of no importance, it is usually considered sufficient to give the mean value and some indication of the closeness with which the individual observations cluster about the mean. This indication may be the probable error, or the standard deviation. The standard deviation, usually denoted by the symbol σ, is such that its square is equal to the average value of the square of the difference between an observation and the mean. Thus σ is a measure of the closeness with which the individual observations cluster about the mean value.

In such an investigation of cycles as I have described earlier we need, not only to consider the value of the standard deviation σ, but also to consider the

[1] *Phil. Trans. Roy. Soc.*, A, 225, p. 247, 1925.


9. Periodogram. Stockholm Mean Temperature
Amplitude in Degrees C.
·5
·4
·3
·2
·1
0
13
14⅔
15⅓
17½
18⅔
19⅓
21
21⅓
24
29
35
42
48
56
66
70
104
180
420
12 Periods in Months 15 16 20 24 26⅔ 30 40 48 60 80 120 240


8. Periodogram. Greenwich Mean Temperature
Amplitude in Degrees F.
·7
·6
·5
·4
·3
·2
·1
0
13
14½
14⅔
15⅓
16⅔
17⅔
19½
21⅓
23
25
25½
26
29
37
42
60
64
88
98
180
210
276
12 Periods in Months 15 16 20 24 26⅔ 30 40 48 60 80 120 240

time relations of the separate observations. The raw material of the investigation was described as a series of deviations of monthly mean temperatures from the normal, and there were used 1200 of these deviations, corresponding to 100 years of observations.

Now suppose that each of the monthly deviations were written on a card, and that the 1200 cards were put into a bag and shaken up, being afterwards drawn one by one. Let the successive drawings be treated as though they were the observations of successive months, and imagine the set of observations so arranged to be investigated for the presence of cycles. Some fairly large amplitudes might well be found in this strangely doctored series of observations, and it is in fact possible to find a value of the amplitude (a value to which Sir Gilbert Walker has given the name of *probable greatest*) such that there is an even chance that at least one of the amplitudes worked from the doctored set of observations shall equal it. This is an obvious standard by which to judge the reality of any period which appears in the original analysis of the observations. If the amplitudes found in a set of observations are not appreciably higher than we should expect to find if we put all the observations into a bag and drew them out in any chance order, then we may surely conclude that the cycles which we have found are of no importance.

Let us consider this theory as applied to the London temperature periodogram of Fig. 8. Theory shows that there would be an even chance of one amplitude as great as 0·47° F. being found if we destroyed the true order of the observations by putting them in a bag and drawing them out one by one, then accepting the order in which they came out of the bag as their order in time. So, when we come to try to interpret the results shown in Fig. 8, we should only regard as important those periods whose amplitudes exceed 0·47° by an appreciable amount. There is not one so great that we can exclude the possibility of its accidental origin, on the basis of the study of Fig. 8 alone. The likeliest to be real are the periods of length thirteen months and twenty-three years. But there are rather more of the amplitudes above or near 0·47° F. than we should have expected on the basis of chance alone.

We are therefore faced with a problem which can be stated in the following way. It is possible that nature works by methods which tend to produce periodicities in temperature, but with only small amplitudes. If this were so, there would appear to be no possibility of determining which these are, on the basis of the discussion of probabilities alone. And in any case, arguing on the basis of probabilities alone is an arid process, since in the end the exact limiting value of the probability which we shall regard as fixing an effective certainty is a matter of taste.

The rather lengthy analysis of the London temperatures to which I have referred above was a part of a detailed analysis of many records. These included the temperatures of London, Edinburgh, Paris, Berlin, Vienna, and Stockholm, the rainfall of Milan, Padua, Edinburgh, and London, and the pressure at Edinburgh and Paris. In not one of the twelve sets of records were the amplitudes of the periods found so great that it was possible to say that they might not have arisen as the result of a chance distribution of the observations.

There are however several other ways of approaching the problem. If the same periods are strongly marked in several places, we shall feel more disposed

to accept these than periods which appear at only one station. It is seen that the periodogram of Stockholm temperatures in Fig. 9 resembles that of London in Fig. 8 in many of its details. The periodograms of temperatures at all six stations mentioned above showed relatively high amplitudes for periods of 13, $14\frac{1}{2}$–$14\frac{2}{3}$, 25–26, 37, and 61 months. But this agreement is to some extent to be expected, since there was very close agreement in the incidence of the cold and warm epochs at the different stations, and the value of the agreement between the six periodograms may be easily over-estimated.

There is a second line of approach, that of seeing whether the same periods show large amplitudes in the records at one place, but over different intervals of time. The periodogram of London temperatures shown in Fig. 8 is based on records for the years 1764 to 1863. Many years ago I worked out a periodogram analysis of the Greenwich records for the years 1841 to 1890.[1] The highest amplitudes during this interval were those for periods of length 19, 23, and 26 months, 4 years, and 5 years. Thus some at least of the peak values agree, though the agreement is not very striking.

A comparison of Figs. 3 and 9 is of some interest. Fig. 3 suggests that the Stockholm records may show some definite tendency to periodicity, but Fig. 9 shows that the only period with a large amplitude is that of thirteen months.

Thus, taking the results of my earlier work for the twelve records mentioned, we are forced to the conclusion that there are few periods, if any, which have such large amplitudes as to inspire confidence in their reality. There is no way of establishing a single period in these records as real, and it is not possible to accept the cycles found in the earlier records as a true indication of what the future will bring.

There is yet another criterion which can be applied to the periodograms which we have been discussing, that of the *utility* of the results. Suppose we have found that a period of any length had an amplitude R in a series of values of the deviations of monthly mean temperatures from normal, this series of deviations having a standard deviation σ. This means that σ^2 is the average value of the squares of the deviations of the individual months from the normal. Now let the individual months be corrected for the part of the variation due to the period of amplitude R. Then it can be shown that the standard deviation of the corrected figures will be $\sigma^2 - \frac{1}{2}R^2$. This process can be repeated for any number of periods, and the square of the standard deviation of the corrected figures will be $\sigma^2 - \frac{1}{2}$ (sum of the squares of the amplitudes used in the correction).

In the London temperatures with which I worked, σ was 3·2° F. Taking all the amplitudes above 0·39° as real, and there were twelve such amplitudes, we find that the square of the standard deviation of the figures obtained by correcting each monthly mean temperature for all twelve of these periods comes out to be $(2{\cdot}95)^2$, or $\frac{6}{7}$ $(3{\cdot}2)^2$. Thus the twelve periods mentioned can only account for one-seventh of the variability shown by the original records. In other words, there are some other causes of variation which are six times as important as the joint effect of the twelve periods whose amplitudes were above 0·39°. This is a mathematically accurate result, which makes no hypotheses of any kind. It agrees closely with a similar result which I published

[1] *Quart. Journ. Roy. Met. Soc.*, 45, p. 323, 1919.

in 1919, in a discussion of periods in the Greenwich temperatures 1841–1890. In this paper I showed that all the possible periods of length between one year and fifty years, no matter what their length, could not account for more than about one-fourth of the variability of the observations. This computation did not take account of reality or otherwise of the periods in the assigned range, but allowed that they might all be treated as real. Thus the fraction one-fourth given above is a considerable over-estimate of the effectiveness of the real periods in the same range.

We are thus led to the conclusion that, whether we regard the periods which appear in the periodograms of Figs. 8 and 9 with relatively large amplitudes as real or not, they can only account for a very small fraction of the variability of the actual phenomena temperatures observed. This test cannot be applied to all the results of other workers, since they often give no idea of the standard deviation of the material with which they work.

6. Periodicity in rainfall

So far the discussion has been restricted to temperature records, but precisely similar results are derived from the examination of rainfall records. In Figs. 5 and 6 above are shown the five-yearly totals of rainfall at Milan and Padua. The harmonic analysis of the data did not show any outstanding amplitude for either of the stations. The highest amplitude in the Milan periodogram was found for a period of forty-nine months, and the highest for Padua for forty years. We can safely draw the same conclusion here as for the temperature records, that no cycle occurs with sufficient amplitude to account for more than a very small fraction of the variability of the rainfall.

7. Brückner's warm and cold spells, and wet and dry spells

We might start off in a still different way, as indeed did many of the prophets who have voiced the opinion that the winter of 1937 would be excessively severe, by taking the cold winters and seeing if they are arranged in any sequence as to dates. It has been suggested by many writers that there is a tendency shown in past records for the lunar-solar cycle of 744 years to appear in the incidence of cold winters. It is difficult to accept any such theory as a serious contribution to the problem we are considering, in the absence of truly scientific records earlier than the eighteenth century.

The cycle which has had the greatest vogue is the Brückner cycle of thirty-five years. Brückner deduced from an examination of European records that there were successions of years which could be classified as warm or cold, wet or dry. His dates were as follows:

Cold	*Warm*	*Wet*	*Dry*
1731–1745	1746–1755	1736–1755	1755–1770
1755–1790	1791–1805	1771–1780	1781–1805
1806–1820	1821–1835	1806–1825	1826–1840
1836–1850	1851–1870	1841–1855	1856–1870
1871–1885		1871–1885	

To facilitate a comparison of these dates with the actual events, I have indicated in Figs. 2, 3, and 4, by means of horizontal lines, the intervals covered by the cold spells, the warm spells corresponding to spaces left blank.

The average interval between the beginning of successive cold spells is 35 years, and for this reason the name of Brückner is associated with a supposed cycle of 35 years, which is usually known as the Brückner cycle. In my periodogram analyses of the twelve sets of meteorological records enumerated earlier there was no single set in which there was a strongly marked cycle of 35 years. In the twelve sets of records the period of length greater than 20 years which had the greatest amplitude in each set came out as follows: 23, 23, 30, 35, 22, 35, 30, 40, 22, 35, 25, and 35 years respectively. It should be pointed out that the only trial periods worked out above 25 years were 30, 35, and 40 years, these being sufficient to ensure that no high amplitude near 30 or 35 years should be missed. For it is impossible for a real period with a large amplitude to have its length within this range without its producing an appreciable amplitude at one or more of the trial periods actually evaluated. The amplitude of the 35-year period was in all twelve sets of records small, and it appears to be justifiable to say that the Brückner cycle is not an important factor in European weather, without any need for discussing the physical reality of the cycle.

Trautmann [1] has recently analyzed a very large number of rainfall records distributed over a large part of Europe, evaluating the amplitudes of trial periods of 31, 32, 33, etc., up to 40 years. He finds that during the interval 1835 to 1925 the period having the largest amplitude might fall anywhere between 31 and 40 years, the average period coming out as 36 years. He only gives the amplitude as a percentage of the average annual rainfall, and gives no indication of the variability of the data with which he works, so that the effect of the period in accounting for the variability of the annual rainfall cannot be deduced from his paper. He finds a fairly regular distribution of the amplitudes and of the phase of maximum of the period of 36 years over a chart showing the stations whose data he analyzed. This does not of necessity prove the reality of the cycle which he investigated, since similar results might be expected to follow from a correlation of the rainfall at one station with that at another, due to the incidence at approximately the same time of relatively wet and dry spells over a wide area. A simple computation with the data for Milan shows that half the square of the amplitude of the 35-year cycle amounts to only 1/56 of the square of the standard deviation of the annual rainfall totals. Thus the 35-year cycle, even if we regard it as real, can only account for 1/56 of the variability of the annual rainfall totals. Or, to put this in another way, other factors which influence the annual rainfall at Milan are 55 times as important as the Brückner cycle.

8. *The sunspot cycle*

It is usual to regard the sunspots as having a cycle of about 11 years, though the actual interval between minima of sunspots may vary within wide limits. The only one of the twelve periodograms which showed an appreciable amplitude at 11 years was Edinburgh temperature, though it should be added that a period of about twice this length, 23 years, had large amplitudes in the periodograms for London and Edinburgh. The lack of strongly marked 11-year periods in the records may conceivably be due to the irregularity in

[1] *Veröff. Geoph. Inst. Leipzig*, IIte Serie, Band VI, heft 6.

the incidence of the sunspot minima. Many years ago a Japanese astronomer worked out a very complete analysis of the sunspot numbers, and drew the curve for a number of future cycles on the assumption that the periods which he had found were real. This occurred some years ago, and it was seen later that the actual records of sunspots had quickly got out of phase with the prediction curve which he had drawn. We must therefore regard the sunspots as phenomena which do not follow accurately any cycle, and it is not to be expected that the average sunspot cycle should be found at all readily in terrestrial phenomena.

The most strongly marked relationship which has ever been found between sunspots and terrestrial phenomena was that between sunspots and the levels of Lake Victoria for the years 1896 to 1921, described by Dr. C. E. P. Brooks in M.O. Geophysical Memoir No. 20, 1923. The maxima of the yearly sunspot numbers in 1907 and 1917 were accompanied by clearly defined maxima of the levels of Lake Victoria, while the levels of Lake Albert also showed a pronounced maximum in 1917. The earlier variations of Lake Albert are not known, and though there was apparently a weak maximum about 1906 or 1907, the lack of earlier observations makes it difficult to specify with certainty the extent of the agreement with the sunspot curve at this epoch. Later observations of the levels of the two lakes do not bear out the close relationship which Brooks found in the earlier observations. In Fig. 10 are shown the data used by Brooks, up to 1921; the data used for the continuation of the curve are taken from a diagram due to Sir Gilbert Walker. The next maximum of the sunspot numbers occurred in 1928, and was followed by a minimum in 1934. The maximum of the lake levels occurred in 1932 or 1933, much nearer to the minimum of sunspots than to the maximum.

Observations are also available of the levels of Lake Michigan, and while these show maxima fairly closely in agreement with the times of the last three sunspot maxima, they show little agreement with earlier maxima.

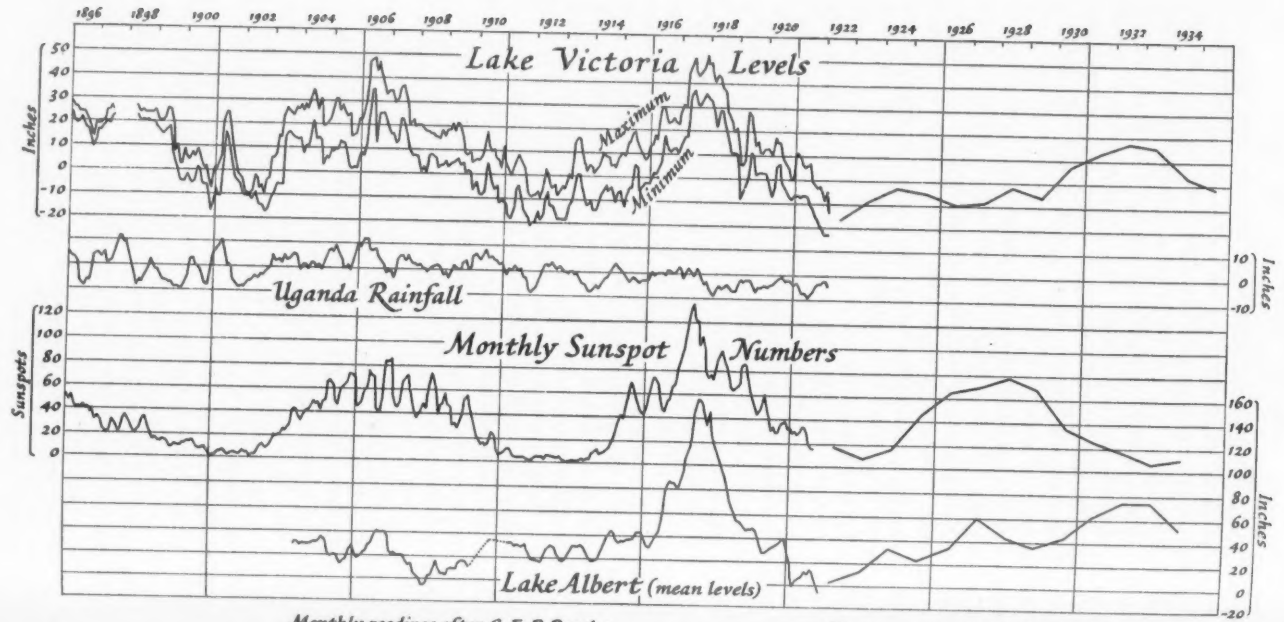
It does not therefore appear to be justifiable to assume that the close correlation found by Brooks between the early data for the lake levels and the sunspot numbers indicates a real physical relationship between the two phenomena.[1]

9. *A further examination of the London temperatures*

It may be helpful to consider in some further detail what have been the changes which have occurred in London temperatures during the interval covered by the data shown in Fig. 2. This diagram shows that the average yearly temperature, after a rapid rise from about 1765 to about 1785, showed a fairly rapid fall from about 1785 to about 1815, and that except for two relapses about 1837 and 1887, it has been steadily rising since 1815. The same steady rise has been manifested in recent years at many other places beside London.

In order to trace in further detail the nature of these large variations in London temperature, I have shown in Figs. 11 and 12 the 5-yearly means of temperature of winter (December to February) and summer (June to August)

[1] Graphs of sunspots and levels of lakes Victoria, Albert, Michigan, and George (Australia) are given by Sir Gilbert Walker in *Quart. Journ. Roy. Met. Soc.*, 62, p. 451, 1936.



Monthly readings after C. E. P. Brooks.

Mean annual readings after Sir Gilbert T. Walker

Fig. 10

respectively. Fig. 12 shows that there has been no appreciable change in the summer temperature during the last 100 years, but Fig. 11 shows that there has been a steady rise in the mean winter temperature during the same interval. The mean temperature of spring and autumn has shown a similar but much smaller upward trend, but their joint contribution to the increase in the mean temperature of the year has been considerably less than that of winter. During the interval 1785 to 1815, the rise and subsequent fall in the mean annual temperature was shared by all four seasons.

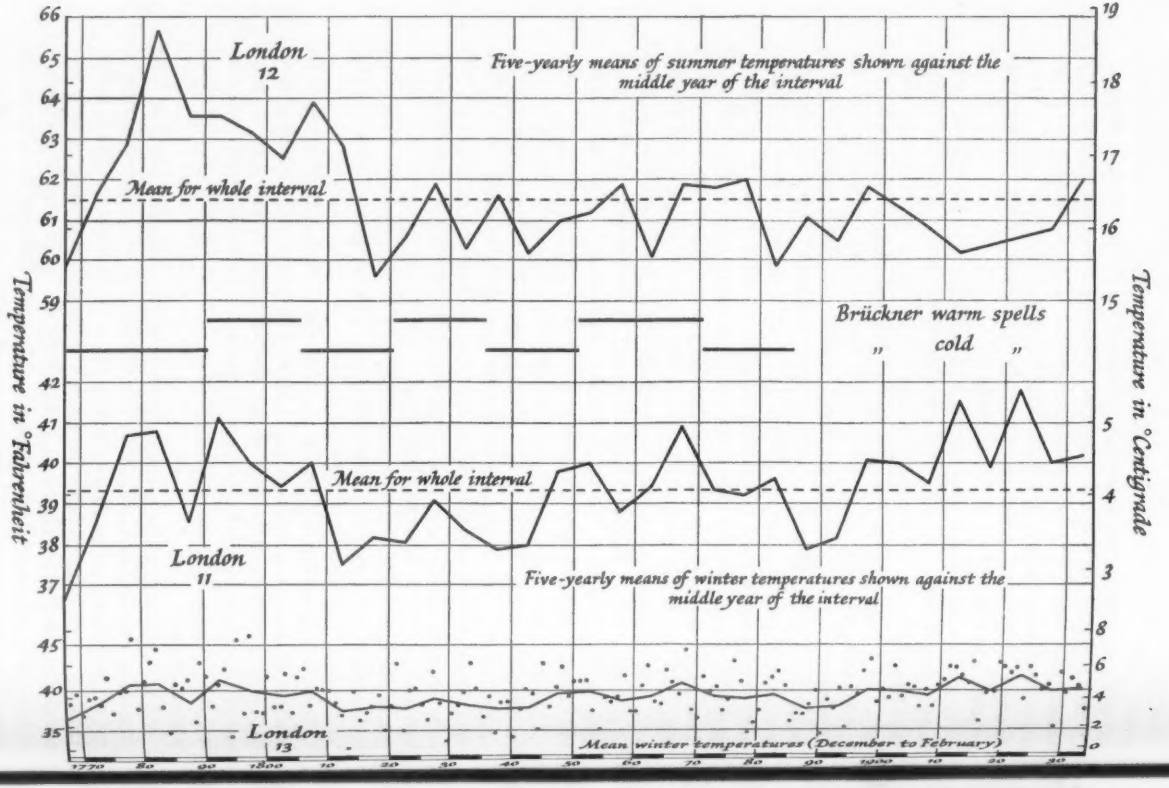
This analysis is carried a step further in Fig. 13, in which the mean temperature of each individual winter (December to February) is shown against the year to which the December belongs. This diagram is worthy of close study. If, for example, we compare the 40 years ending at 1933 with the 80 years ending at 1893, we find that while the "warm" winters in the two intervals are about equally mild, the "cold" winters were both colder and more frequent in the earlier interval. In the 40 years ending at 1933 there were only two months with a mean temperature of 32° F. or lower, while in the 80 years ending at 1893 there were 11 months with temperature of 32° F. or lower; and while the last 40 years had only two months with a temperature between 32° and 35° F., the previous 80 years contained 31 months whose mean temperature fell within the same limits. If, on the other hand, we look for years in which no month had a mean temperature below 40° F., we find 4 in the last 40 years, and 9 in the preceding 80 years. There has been no very great change in the position of the band within which most of the winter temperatures are contained in the diagram.

If in Fig. 13 we take the year 1850 as the middle point of the record, we find that of the winters which had a mean temperature of 34° or lower, all 5 occurred before 1850, none occurring after that date, while of the winters which had a mean temperature of 35° or lower, 10 occurred before 1850, and 2 after 1850. It therefore appears that the only appreciable change which has taken place in the last 120 years or so in the climate of London is a progressive decrease in the frequency and intensity of cold winters. There has not been any corresponding marked increase in the frequency of unusually warm winters, as is seen from Fig. 13.

These conclusions lend no real support to the idea that the variations to which the climate of London is subject can be regarded as cyclic in their nature, if we are to use the word "cyclic" in any normal sense. I cannot say to what extent the results shown in Figs. 12 and 13 are also to be found at other stations in Europe, as no similar analysis has been carried out for other records than those of London.

In Fig. 14 are shown the 5-yearly mean pressures recorded in Paris. The curve shows a decided swing downward in the first half of the interval covered by the observations, followed by a steady recovery to about the initial value. This diagram suggests that one factor in the variations of temperature and rainfall over the British Isles was a change in the mean pressure, which is probably to be interpreted as a change in the tracks followed by depressions coming from the Atlantic. The minimum at about 1817 corresponds to a minimum of temperature shown in Fig. 2, and this minimum was also evident in the temperatures of Edinburgh, Paris, and Berlin. Dr. C. E. P. Brooks and

Fig. 10


London
12
Five-yearly means of summer temperatures shown against the middle year of the interval
Mean for whole interval
Brückner warm spells
" cold "
Mean for whole interval
London
11
Five-yearly means of winter temperatures shown against the middle year of the interval
London
13
Mean winter temperatures (December to February)
Temperature in °Fahrenheit
Temperature in °Centigrade
66
65
64
63
62
61
60
59
42
41
40
39
38
37
45
40
35
19
18
17
16
15
5
4
3
8
6
4
2
0
1770
80
90
1800
10
20
30
40
50
60
70
80
90
1900
10
20
30

Miss T. M. Hunt [1] have shown that this was a period of marked increase in the frequency of east winds in winter, and this confirms the suggestion that during this period there was a marked change in the tracks followed by the depressions affecting the British Isles.

10. *Periodicity in non-meteorological records, tree-rings, and clays*

So far we have only considered instrumental meteorological records. Other types of data which have been investigated for cycles are tree-rings, and the thickness of "varved" deposits of clay.

The investigation of tree-rings is largely the work of Dr. A. E. Douglass, of the University of Arizona. Some trees have been found which show as many as 3000 rings of annual growth, and by measuring the thickness of the individual rings Douglass has obtained a series of data which represent, though

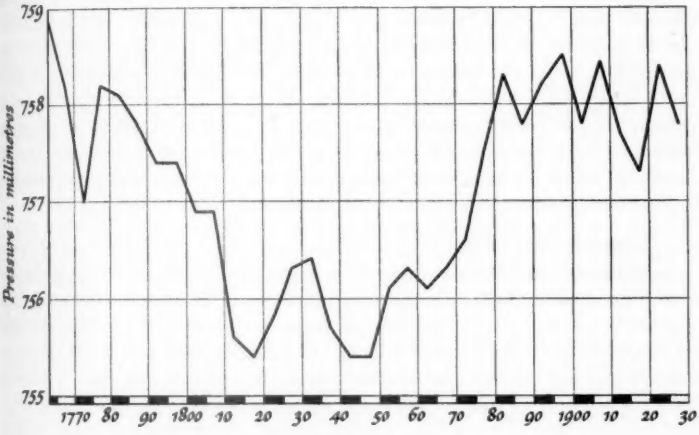


Fig. 14

perhaps in a rather indefinite way, the meteorological conditions during the whole life of the tree. Trees are usually selected from regions near the limits of the zone of growth, so that the occurrence of unusually favourable conditions may be reflected in an unusually large amount of growth, shown by a wider annual ring of growth than occurs in the normal year.

Douglass claims to find a marked cycle of 11 years, one of one-half this length, 5·7 years, one of 14 years, and one of 21 years, not all of necessity present at the same time in one tree. The present writer does not find the results as presented very convincing, but he makes no claim to understand the details of the methods used by Douglass. It is to be hoped that others taking part in this discussion will be able to speak on this topic with more authority.

The variation in the growth of trees is apparently regarded as being in the main due to a variation in the annual rainfall. But it must be added that the

[1] *Quart. Journ. Roy. Met. Soc.*, 59, p. 375, 1933.

cycles discussed by Douglass and enumerated above do not show any prominence in the rainfall records of Europe. The periods of 5·7, 11, 14, or 21 years are not to be found in the rainfall records of Milan, Padua, Edinburgh, and London. A fairly large amplitude was found for 11 years in the Edinburgh temperatures, and a moderate amplitude, a little over 0·3° C., for 14 years in the Vienna temperatures.

Efforts have also been made to trace periodicity in the thicknesses of the so-called "varved clays." These clays are of glacial-marginal origin, being deposited in lakes fed from the front of the continental ice-sheet. The coarser sediment is deposited near the shore, while the fine clay remains in suspension for a longer time, and is eventually deposited during the following winter as a dark band of clay. The deposits so formed are thus markedly banded in character, and the thicknesses of the bands show considerable differences among themselves. As the clay is originally carried into the lake, at least in part, by water formed by thawing of the glacier in summer, the thickness of the clay should be a measure of the weather conditions in summer, being greater the hotter the summer. This simple relationship is probably in part complicated by the rainfall, so that the actual thickness of a layer is not a measure of the summer temperature alone. It is doubtful whether enough is known of the precise conditions which govern the laying down of the varved layers to make it possible to draw any final conclusions as to the interpretation of the measurements of thicknesses of the varved clays.

11. *Summary and conclusion*

In the above paragraphs a description has been given of results derived from a very detailed analysis of meteorological records, in a search for cycles. It is shown that none of the cycles found occur with such great amplitudes that we can assert that they might not be due to a chance distribution of the observations, and not to truly periodic variations. It is shown that even if some of the cycles found are real physical cycles, they can only account for a small part of the variability of the meteorological phenomena; so small a part indeed that it is of little importance whether the so-called cycles are real or not.

The variations of the mean temperature of summer, winter, and the year, in London, are discussed by means of diagrams of the observations, and it is shown that the main change which has taken place in the climate of London in the past 120 years is a steady decrease in the frequency and intensity of cold winters. The variations in the mean pressure of Paris are also shown diagrammatically, and it is inferred that there was a marked change in the track of depressions coming from the Atlantic during the early years of the last century.

DISCUSSION

Before the paper the PRESIDENT (Professor HENRY BALFOUR) said: Professor Brunt in lecturing to us this afternoon will be dealing with a subject of extraordinary interest but very complicated, and as it is a subject which will undoubtedly produce a lengthy discussion I do not now propose to delay the proceedings. I therefore formally introduce Professor Brunt to you, if that is necessary, which I very much doubt, and call upon him at once to give his lecture.

Professor Brunt then read the paper printed above, and a discussion followed.

The PRESIDENT: I ask Dr. Brooks to be good enough to open the discussion.

Dr. C. E. P. BROOKS: I listened to Professor Brunt's paper with great interest. I have dabbled in periodicities, though not at all systematically. My first slide [not reproduced] deals with the rainfall of England, but instead of taking single stations I used a mean for the whole country over the period of two hundred years. An interesting point is that I made my analysis early in 1928 and carried it on for a few years, and although I was most careful to warn my readers that it was not a forecast, actually the curve did reproduce the rainfall of those years fairly well. If I had not given that warning, probably it would not have done so!

I should like to make some reference to Douglass's tree-ring data, because it seems to me they may serve a very useful purpose. Douglass speaks of discovering the normal succession of cycles. It has happened over and over again that just when some weather cycle seems to be well established, like the three-year one in the British rainfall, it stops suddenly. It is necessary to ascertain the reason for that before we can make any use of periodicity. Ordinary instrumental observations do not give long enough series, but in the 3000- or 4000-year tree records we may find what we are looking for.

In dealing with these long records however it may prove a hindrance to hold fixedly to the mathematical idea of cycles of invariable length. I can show an example by putting on the screen a slide [not reproduced] in reference to the Nile Floods.[1] I analyzed over 600 years' records for periodicities, and if I had looked for cycles of fixed length I should never have found any trace of a 22-year cycle; but as I did it in small sections and compared the results, I found there really was a 22-year cycle, but it varied very slightly in length in a systematic way. If one adopts as a definition of a 22-years' cycle that it should recur at exactly the same length, there is no such cycle, but the regular oscillation of the phase angles suggests that there is something there. If you do not call it a cycle I do not know what you can call it.

In his paper Professor Brunt says that the lake-levels are the most marked relationships to be found between sunspots and terrestrial phenomena. I do not think that is quite true. I suppose the best relation is that with terrestrial magnetism. Even in meteorology thunderstorm records in Siberia give a decidedly better relation with sunspots than the African lake-levels do, and it goes on for longer periods than the lake-levels. I was not able to get figures subsequent to 1924, but other researches gave a similar relation between thunderstorms and sunspots over a period of more than sixty years, in high latitudes and in equatorial regions. Whether it will go on any longer I do not know. Probably having advertised it, it will now stop.

Professor Brunt referred to my work on wind. That, I think, is another element which would repay study of periodicities. It has two advantages. The first is that you do not always know why there is a cold winter. It might be cold due to east winds or due to an anticyclone, whereas if you study the wind direction you are one step nearer to the cause. The other point is that wind directions really give a double series. You have the north component and the east component. If you analyze those separately and find the same periodicities in both, then that is some sort of a check. I have about 150 years of winds and have worked out periodicities for those.[2] Both north and east components showed marked cycles of 51 years and 3·0 years. The 51-year cycle fits in with the long swing of rainfall. Heavy rainfall came in the periods

[1] "Periodicities in the Nile floods," *Mem. Roy. Met. Soc.*, 2, no. 12, London, 1928.
[2] *Meteor. Mag.*, 68, p. 154, London, 1933.

of west wind and the least rain came with the periods of east wind. You may remember that one of the cycles of temperature which Professor Brunt thought was real was one of 61 months; this occurs also in the north component of winds. It was not found in the east component, but obviously the north and south swing of the winds will explain the temperature cycle very neatly. There was also a cycle of 22 years in the east component but not in the north component.

Finally, Professor Brunt's explanation of the decreasing frequency of cold winters really comes down to a combination of a rise of mean temperature in winter with a decrease of variability. That decrease of variability in the past forty years or so has been very widespread over the greater part of the northern hemisphere, at any rate in middle latitudes, and it almost looks as if forty odd years ago something happened which upset the world meteorologically and that we have been settling down ever since. That may be the reason why weather cycles have been so elusive. If so, it is possible that, in the future, periodicity hunting may be more fruitful.

Mr. HAWKE: In the third paragraph of this most interesting paper Professor Brunt seems disposed to admit that the early observations of temperature may not be entirely reliable. He says that we can only accept them as they stand, "particularly as they are, to some extent, confirmed by the general descriptions of weather . . . which are available." He refers, presumably, to archives and contemporary diaries. I should like to raise the question whether there is, in fact, any such confirmation for the remarkable period of excessive summer heat which is shown between 1781 and 1785 in Professor Brunt's diagram. Everybody here must be familiar with Gilbert White's 'Natural History of Selborne.' Now White was, of course, a very careful observer, not only of trees, plants, birds and animals, but also of the weather. He kept a record of rainfall at Selborne from 1782 to 1793, as well as a pretty full meteorological log from 1768 to 1792. In or shortly before the year 1788 he wrote: "On chalky and sandy soils, and in the hot villages about London, the thermometer has been often observed to mount as high as 83° or 84°; but with us, in this hilly and woody district, I have hardly ever seen it exceed 80°; nor does it often arrive at that pitch." This does not in the least suggest that there had been a run of extremely hot summers in South-East England a few years before. Temperatures of 83° and 84° F. are nothing out of the ordinary nowadays in and around London. We do not begin to talk about "heat-waves" until the thermometer gets to near 90° F. In the two hottest spells of the twentieth century 100° and 99° F. were the absolute maxima at Greenwich Observatory.

According to Alexander Buchan's data, which Professor Brunt has used, the average summer temperatures (June to August) in London, reduced to 24-hour means at Greenwich, were:

1781	66·2° F.
1782	66·1
1783	70·3
1784	64·8
1785	65·7

The two hottest summers (June to August) since 1841 at Greenwich gave means of approximately 65° F., so that the value of 70·3° F. entered to 1783 represents a sustained pitch of heat far beyond anything in more modern experience.

Here are Gilbert White's descriptions of the summers from 1781 to 1785. For 1781 he gives: "June began with heavy rain, but thence to the end of

October dry weather with a few flying showers." For 1782: "June warm and dry; to the end of August, warm, with almost perpetual rains." For 1783: "from May 10 to the end of August, hot weather with frequent showers." White was always looking at his thermometers, but he quotes no temperatures for the summer of 1783, as he almost certainly would have done if there had been any exceptional readings. For 1784 he says: "to June 27, warm, with frequent showers; to July 18, hot and dry; to the end of August, warm, with heavy rains." For 1785: "to June 9, mild, windy weather with frequent soft showers; to July 13, hot, dry weather with a few showery intervals; to July 22, heavy rain; to the end of September, warm weather with frequent showers."

Clearly, White's log provides no reason for supposing that those five summers, 1781–1785, were excessively hot; and if, as he leads us to suppose in the passage quoted above, they yielded no temperatures much above 80° F. at Selborne, they certainly were not at all exceptional by modern standards.

Alexander Buchan was a great meteorologist, but he was, perhaps, a little lacking in discrimination, and at times he was apt to be somewhat rash, as I have reason to know, because I happen to have spent the last six months writing a book on his life and work. Personally, I cannot help feeling that there was something radically wrong with some of the temperature data which he used for determining the monthly means for the last few decades of the eighteenth century, and I think he would have been wiser to reject them. Until about 1790 they had only the old combined maximum and minimum registering thermometers of Cavendish and Sixe—types of instrument which, as all meteorologists know, are liable to get out of order, and which have long since been superseded for accurate scientific work.

In regard to the so-called sunspot cycle and the alleged connection between it and the eleven-year periodicity found by Dr. Douglass for the tree-rings, I wonder if Professor Brunt could tell us whether Douglass detected anything abnormal in tree-ring growths between 1645 and 1715. During that period there must have been a notable dwindling of solar activity. It is known that very few sunspots large enough to be seen by the naked eye appeared in all those years, and according to some astronomers the sunspot cycle was then practically in abeyance.

The PRESIDENT: Sir Gilbert Walker is unable to be present, but he has sent a letter which I will ask the Secretary to be good enough to read.

Sir GILBERT WALKER wrote as follows: I am extremely sorry not to be present at Professor Brunt's lecture, for I regard the widespread faith in the effective control of weather by periods as based partly on a mistaken handling of plotted data and partly on an instinct that survives in many of us, like the faith in the effect of the moon on the weather, from the time when our forefathers believed in the control of human affairs by the heavenly bodies with their fixed cycles.

The way in which plotted data prove misleading is that in many countries the seasonal characteristic of one year tends to persist and produce a similar characteristic in the following year; thus the graphs show surges of abundant rainfall lasting several years with intervals of deficient rain between them. A plot of seventy years will in general contain four or five of these surges, and it will be surprising if chance does not make it possible to fit them into some rough scheme of cycles.

I do not know a single period, other than those of sunspots, of which the reality is clearly established by the necessary use of the mathematical criteria: and some of the period-finders seem to recognize this, for one at least has said that he has ceased to pay attention to criteria because when he did so he obtained scarcely any results of interest.

I agree with the lecturer that, even if some of the periods claimed should ultimately prove to be real, their effects are too small to be of use in forecasting. The Indian monsoon, for example, has clear associations with the Nile floods and with other important seasonal rainfalls for which control by cycles is claimed; yet we had in 1917 the biggest excess of rainfall on record, and in 1918 almost as big a deficiency. Surely any domination by a period of 9, 11, 16, 19, 23, or 37 years, to mention a few of those claimed, could not produce such big opposite departures in consecutive years. Further, towards the end of the last century, seasonal forecasts were issued for a number of years in Australia and in South Africa, based on a system of two or three cycles; and in both countries they were given up because of their conspicuous failure.

Mr. HINKS: There are two questions I may be allowed to ask. First, I should like to know whether Professor Brunt can assure us that the foundation of the celebrated cycle of Brückner was no more than is represented by those few lines in one of his earlier diagrams. If so, it is a most forbidding example of the ease with which a man can sometimes get away with building up a big edifice on a very small foundation.

Secondly, I should like to ask Professor Brunt whether he thinks that in these questions of correlation between sunspots and other phenomena there is anything in the theory which has been put forward, that the real period we should deal with is not eleven and a half years but twenty-three years. I believe I am right in saying that one of the most striking things found in Solar work at the Mount Wilson Observatory was the reversal of the magnetic field in sunspots, by which they have one polarity for a period of eleven and a half years and then the polarity is reversed for the next period. That would hint that the eleven and a half years he has been dealing with is the half-period and not the whole period which has to be taken into account when we are considering the possible action of the Sun upon the Earth.

Dr. DUDLEY STAMP: I have no special knowledge of the subject but I want to ask a question or two. One I had in mind has been partly covered by Sir Gilbert Walker's remarks. The cycles one would expect are those *irregular* in period. As Professor Brunt was speaking, my mind went to my colleagues the economists and their trade cycles. I think they would be rather surprised if they determined a regular periodicity, so that they could forecast the trade cycles of the future. I think I am right in saying that the height and depth of the boom and depression respectively are related to the length of time involved, just as with waves on the sea shore. May I ask if any attempt has been made to analyze the figures of temperature records on that basis?

Secondly, in analyzing the meteorological data, it rather seemed that simple climatology had been neglected. Surely, for example, to attempt a comparison between London and Stockholm is to ignore all that one learns in elementary climatology, that those two stations are in different climatological regions. To take one simple example: winter conditions established over eastern Europe may be of such a character that the normal winter high-pressure area of eastern Europe is very extensive and exerts its influence as far as eastern England. Then one will get a possible comparison between the temperature figures of London and Stockholm, but there will be a direct *contrast*, more marked than usual, between, let us say, London and north-western Scotland, in that the latter is outside the high-pressure system and, lying in the direct path of cyclonic disturbance, will follow along that unusual line. Therefore, one would not expect to find correlation between the temperature and rainfall curves at various stations in Europe, but rather a greater contrast according to the climatic regions in which they are situated. Taking the world as a whole, one would expect to

find unusually excessive precipitation in one climatic region balanced by exceptional drought in another, and therefore the "cycles" determined in one area complementary to rather than directly comparable with those in another.

I have not had an opportunity to study the paper beforehand, and I feel that I have not put my points very clearly, but perhaps sufficiently clearly for Professor Brunt to enlighten me in those two respects.

The PRESIDENT: Professor Brunt has been asked many questions, to which I hope he will reply. His lecture has certainly been stimulating, and his criticism largely destructive, but none the less telling for that. I imagine that there is still some hope for cycles, in spite of the destructive criticism to which they have been subjected.

When I knew that Professor Brunt was going to discuss the question of cycles I was rather hoping that he might be tempted to deal with some of those great oscillations, perhaps I had better call them, of climate which have been repeated and recorded not by man's records but by Nature's own deposits. I am referring to those extremely interesting problems which arise from the so-called glacial periods—periods when the Ice Cap advanced over northern Europe and receded; then advanced again and receded, and once more advanced and receded. And corresponding with those oscillations in the north there is the question of the possible correlation of excessive and successive pluvial periods in the south, notably as they have been observed in Asia and Africa in the neighbourhood of the great lakes. There was brief reference to the great lakes during the course of the lecture, but that particular question of the cyclic oscillation of climatic conditions did not actually arise. These are however major phenomena which are of extreme interest to a great number of people—biologists, palaeontologists, and archaeologists are all concerned with the possible correlation of pluvial periods in the East African region, or elsewhere for that matter, and the glacial periods which dominated our own part of the world in successive periods. If these can be correlated and in any way synchronized, then the archaeologists will have a great deal to thank the meteorologists for.

I do not know whether I can tempt Professor Brunt to say a word on that point, and to offer any kind of hope of a synchronization being arrived at which will be usable as a basis for dealing with pre-historic sequences and other matters concerned.

Prof. BRUNT: Dr. Brooks has maintained that the period of fifty-one years which he deduced for British rainfall appeared to be the most important of all those which he investigated. In my own investigation of Greenwich temperatures for 1841–1890 the trial period of fifty years had an amplitude of 0·49° F. I did not consider it seriously, as one would require a period to be repeated several times before accepting it as real. In the analysis of the other records of temperature, rainfall, and pressure which I have described in my paper I did not go beyond thirty-five years in the lengths of trial periods, for the same reason, that I wanted to restrict myself to trial periods which had been repeated several times in the interval covered by the observations. The variable period in the Nile Floods which Dr. Brooks has described is unique, in that no similar results have ever been found in other records, at least to my knowledge. I am quite in agreement with Dr. Brooks as to the value of observations of wind direction. Periodicity in these would be easier to interpret than those in temperature or rainfall.

Mr. Hawke has thrown some doubt on the validity of some of Buchan's observations of temperature in London. But while I accept Mr. Hawke's criticism, I do not consider that it has any great effect on the results which I have derived. The agreement between the periodicities in twelve sets of

observations suggests that the effect of having erroneous values for some twenty years of London temperatures has no appreciable effect on the computation of the periodicities.

Mr. Hawke drew attention to the dearth of sunspots during the interval 1645 to 1715. Douglass says that during this interval the sequoias showed no 5·7-year period, but an approximate 10-year period. As I said in describing my paper, I lay no claim to serious knowledge of the factors which influence the growth of trees, and I had hoped that in the course of the discussion some one would have thrown light on this topic. I hesitate to say that the occurrence of a particular cycle in the growth of trees indicates the presence of that cycle in rainfall or in temperature. It indicates a cycle in some complicated function of the climatic conditions, and for that reason I am not surprised that the cycles which Douglass finds in trees do not show up in my analyses of temperature or rainfall.

Dr. Whipple raised the question which has always puzzled me: What basic cause is there which can lead to such odd periods as 37 or 61 months? It was the impossibility of finding some reasonable physical cause for the periods shown by the harmonic analysis that caused me to abandon the study of this subject, which I had not intended to touch again. But it is difficult to imagine anything in the atmosphere which can be regarded as subject to a variation in such periods as 13, 19, 26, 37, 42, or 61 months. I agree also with Dr. Whipple that there is no special reason for assuming that any periodic variations in the atmosphere should be of necessity given by sine-curves, but the advantages of using the usual method of harmonic analysis are almost overwhelming.

The sunspot cycle is an example of the difficulty of finding a satisfactory method of analysis. Many years ago a Japanese astronomer worked out an elaborate analysis of the sunspot data, and drew a curve to show the future variations of the sunspots, on the assumption that the same periods, represented by sine-curves, would hold in the future as in the past. After a very short time it was found that the actual data got completely out of phase with the predicted curve, giving a maximum when the predicted curve gave a minimum. It appears, in fact, that we cannot in such matters assume that to-morrow will be governed by the same procedure as to-day.

Sir Gilbert Walker emphasized a point to which reference was made in my paper, that while variations of long period should give flat peaks in a curve the actual observations of temperature and rainfall show sometimes a year of excess followed by a year of marked defect. It is difficult to see how such sequences can be conditioned by periodic variations.

I may have misled Mr. Hinks as to Brückner's work. Brückner discussed data for practically the whole of Europe, when he fixed his warm and cold, wet and dry spells. But at any individual station the fit of the Brückner spells to the observations is usually unsatisfactory.

Dr. Stamp questioned the possibility of the occurrence of economic cycles of constant length and amplitude. Now meteorologists have in the past carried out lengthy computations of periodicity on the assumption of constancy of length of period and of amplitude. The meteorologists have in fact made the assumption that to-morrow will be conditioned by the same variations as to-day, and in general when they speak of cycles they refer to cycles of this nature. I do not believe that such cycles would ever be found in economic data. Dr. Stamp urged that as London and Stockholm are in different climatic regions, it was not to be expected that the results derived from observations at these two places should show any agreement. But as I showed in the diagrams of Figs. 2, 3, and 4, there are considerable similarities in the large-scale variations at places

widely separated, and the periodogram analyses show that the same apparent periods occur at the different places.

The President has asked me to say something about the problem of the very large-scale variations of climate associated with the ice ages. Many efforts have been made to explain these phenomena, some of them on the basis of an assumed variation in the amount of heat coming to the earth from the sun. The earlier writers who attempted an explanation on this basis assumed that the colder epochs were caused by a decrease in the solar radiation; but the opposite view, that the colder epochs in high latitudes were caused by an increase in the incoming solar radiation, was first put forward, so far as I know, by Sir John Herschel, in an essay entitled "Weather and Weather Prophets," published first in a little journal entitled *Good Words*, and afterwards included in a volume of collected essays, published in 1866. Simpson has developed this view in far greater detail, and I shall here give a very brief account of Simpson's theory, as he has stated in a paper in the *Quarterly Journal* of the Royal Meteorological Society, vol. 60, 1934, p. 425.

The general circulation of the atmosphere may be regarded as conditioned by the difference of temperature between high and low latitudes, so that if this difference is increased the circulation will be speeded up. Simpson starts off with the assumption that the incoming solar radiation is subject to a periodic variation such as that shown in curve 1, Fig. 1. Starting from the time of minimum radiation, he follows the changes of the solar radiation through a whole period of variation. With the initial increase of radiation, there will be a general rise of temperature over the whole earth, and an increase in the difference of temperature between equator and polar regions. This will lead to an increase in the energy of the general circulation of the atmosphere, which will show itself in increased winds, leading to an increase in cloud and precipitation. The increase of precipitation will involve an increase in the amount of snow falling at high levels and in high latitudes, while the increase of cloud amount will prevent summer melting of the snow. Thus the polar ice caps should spread southward, while ice fields should extend to lower levels. This should eventually lead to what we know as an ice age. If however the increase in solar radiation is progressively maintained for a long time, the general rise of temperature will lead to the melting of the ice, and to a diminution in the amount of snow which will fall in winter. Thus the epoch of maximum of solar radiation will be an interglacial epoch marked by high temperature and high rainfall. With the progressive decrease in solar radiation from the maximum down to the next minimum the process will be reversed, an ice age occurring somewhere on the downward slope to the next minimum, and finally a cold and dry interglacial epoch at the minimum. Thus on Simpson's theory the time of minimum solar radiation will be a cold and dry interglacial epoch, the time of maximum solar radiation will be a warm and wet interglacial epoch, and two ice ages will occur within the whole cycle, one on the ascending part of the curve, and one on the descending part of the curve. In the unglaciated regions a pluvial epoch should occur during the time when the general circulation of the atmosphere is speeded up, and should therefore extend over two ice ages and the intervening warm and wet interglacial epoch.

The theory as stated is a very plausible one, but it has the defect that it is impossible either to prove or disprove it. It is not possible on the basis of meteorological observations to make any test of this theory, as the interval covered by even the longest series of meteorological data is negligibly small by comparison with the length of time represented by the length of the period of solar variation which the theory demands. Any test which is made will have to

be on the basis of geological facts, and it is questionable whether geology will be able to present facts in a form capable of testing the theory during the present century. The length of the period of solar radiation which Simpson predicates is equal to the interval between the middle of the interglacial epochs separating the Gunz and Mindel, and the Riss and Würm glacial epochs. That the sun should have so long a period of variation is not impossible, so far as we know. Astronomical theory can give no guidance in this matter. The total variation in the amount of the solar variation which Simpson requires is less than that shown by many known variable stars.

At the meeting of the Royal Meteorological Society at which it was discussed Simpson's theory met with considerable opposition from many of the geologists present. It was also pointed out that the theory does not give any explanation of the genial climates which are said to have occurred in the Triassic, Jurassic, and Tertiary.

Simpson's theory is extremely plastic, and could be modified to fit a wide variety of facts. Thus, if the assumed variation of the sun were smaller than Simpson predicates, it appears possible that the ice age would form further up the slope of the radiation curve, and might occur at the epoch of maximum radiation, so that we should then have the two ice ages represented by Simpson as occurring in each period of solar radiation now coinciding at the epoch of maximum radiation. This plasticity of the theory makes it less useful for the classification of geological facts than it appears at first sight.

In conclusion, I should like to thank you for the kind reception which you have given to my attempt to deal with a very complex and far-reaching subject.

The PRESIDENT: Our thanks are also due to Professor Brunt. I am sure you would like to express them before we close the meeting.

THE ARUN RIVER DRAINAGE PATTERN AND THE RISE OF THE HIMALAYA

L. R. WAGER

THE Arun river collects its waters from the north side of the Himalaya and from the south side of the lower indefinite range known as the Nepal–Tibet watershed. After flowing approximately eastward along the grain of the country it turns abruptly south and in a series of magnificent gorges cuts through the Himalaya between Mount Everest and Kangchenjunga. The remarkable behaviour of the Arun and several other rivers in cutting across from the plateau of Tibet through the much higher Himalayan range has frequently been commented upon, and two alternative theories, with very different implications for the student of the geology and geography of the Himalaya, have been put forward to explain it. One of the theories postulates that at an early stage the Himalaya had ordinary consequent drainage, the rivers flowing north and south from the crest. This simple drainage pattern is considered to have been modified to its present form by some of the south-flowing rivers cutting back through the range and capturing rivers on the Tibetan side. The much greater precipitation on the south side of the range and the much steeper fall and therefore greater erosive power are put forward as possible reasons for the unusual behaviour of the south-flowing rivers. The alternative theory postulates that the Arun and similar rivers always had their present courses which, when they were inaugurated, were the easiest routes down an irregular surface sloping towards the Gangetic plain. Subsequently the Himalayan range is considered to have risen up across the rivers, but so slowly that by vigorous erosion they were able to keep open their original channels. If this latter theory of antecedent drainage, as it is called, could be substantiated, it would provide definite evidence of an important early stage in the surface form of the Himalaya, and it would also give a definite indication of the mechanism by which the Himalaya has grown to its present height.

The theory that the Arun has cut back through the Himalaya and captured Tibetan rivers has been supported by Hayden [1] and Heron.[2] Fox,[3] in a discussion of Heron's paper, and Odell [4] have tentatively suggested the alternative view. Many others, especially Medlicott, Oldham, and Burrard,[5] have discussed similar cases or the general case of which the Arun is an

[1] Hayden, H. H., "The geology of the provinces of Tsang and U in Central Tibet," *Mem. Geol. Surv. India*, vol. XXXVI, Part 2, 1907, p. 5.

[2] Heron, A. M., "Geological results of the Mount Everest Reconnaissance Expedition." *Mem. Geol. Surv. India*, vol. LIV, Part 2, 1922, pp. 215–34, and *Geogr. J.* 59 (1922) 418–31.

[3] Fox, in the discussion of Heron's paper, *Geogr. J.* 59 (1922) 431–6.

[4] Odell, N. E., "Observations of the rocks and glaciers of Mount Everest," *Geogr. J.* 66 (1925) 300. And Appendix in 'The fight for Everest,' pp. 305–6, 1925.

[5] Summaries with references are given in Burrard, S. G., and Hayden, H. H., 'A sketch of the geography and geology of the Himalaya Mountains and Tibet,' revised by by S. G. Burrard and A. M. Heron, Delhi, 1934, pp. 261–6 and 347–9.

example, but the evidence has not been sufficient to come to a final conclusion. During the 1933 Mount Everest expedition there was an opportunity of seeing much of the Upper Arun, and I propose to give here some general observations on the physiography of the region and then to describe certain features which I believe afford definite evidence that the Himalayan range has risen across the course of the Arun without modifying to any significant extent the original direction of the river.

The Upper Arun and its tributaries

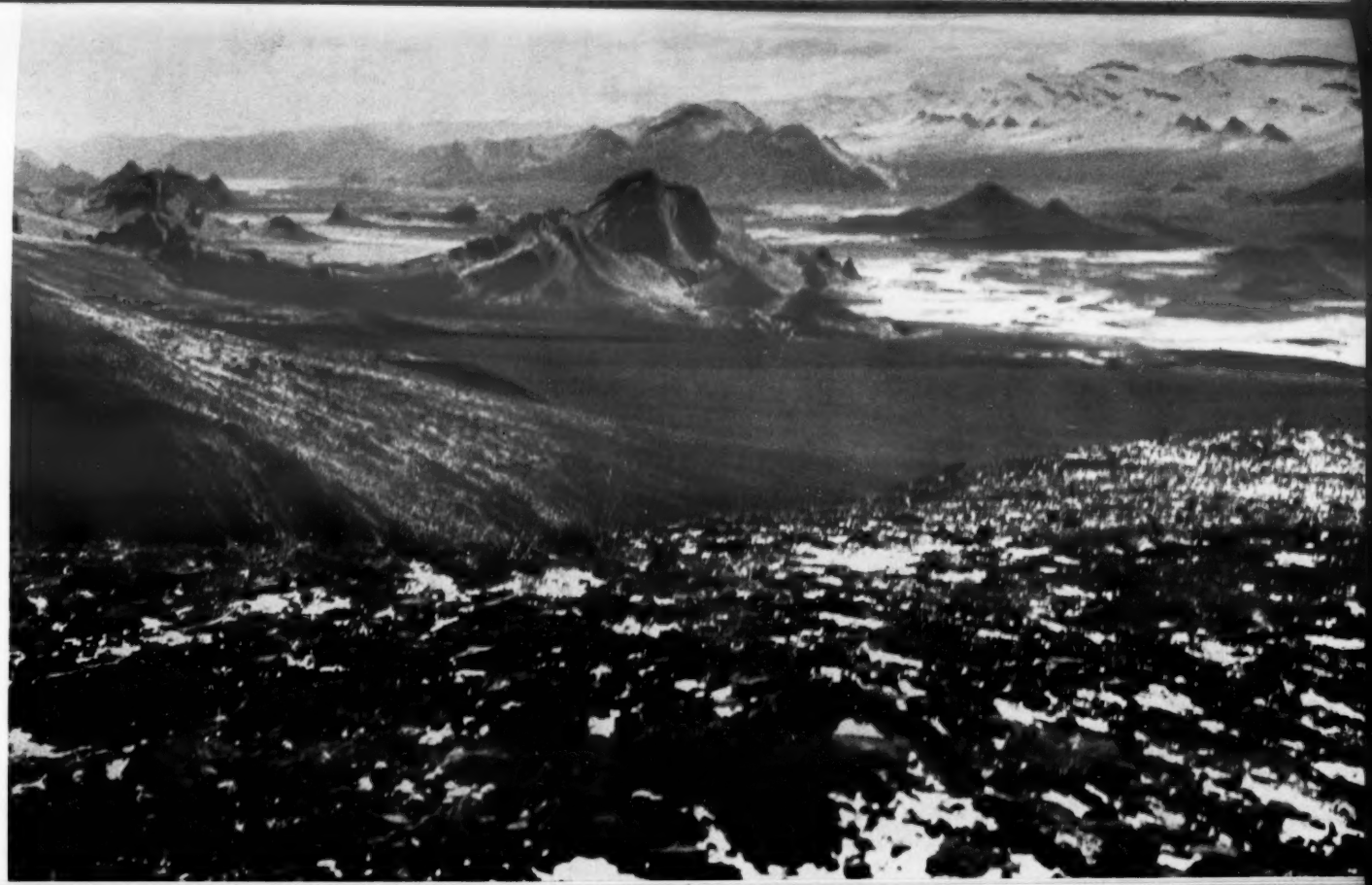
The relief of the mountain country through which the Arun flows can be most satisfactorily shown on a small-scale map by giving spot heights along the course of the river and indicating the position of the high land by marking all peaks of certain height intervals. Thus on the folding map all peaks between 19,000 and 22,000 feet are shown by small triangles, and all peaks above 22,000 feet by larger triangles. By choosing these particular heights the position of the indefinite range, the so-called Nepal–Tibet watershed[1] which forms the northern limit of the Arun drainage system is well brought out, while the main Himalaya is shown as a range with many peaks higher than this. From the Survey of India maps on the scale of 4 inches to 1 mile[2] the profile of the river can be drawn (see folding map). The actual profile is very different from that of a graded river, the form given in a misleading diagrammatic section by Burrard.[3] An abrupt increase in gradient occurs where the river leaves the plateau and begins to cut through the Himalaya, and it is here, soon after the point where the valley floor becomes convex, that the Yo Ri gorge, the first of the striking gorges through which the Arun flows, is found. The main gorge begins at Kharta Shika and with occasional broadenings continues for the next 50 miles. Twenty miles beyond this point the Arun joins the Kosi, then shortly reaches the southern boundary of the Himalaya; here the river is at a height of only 200 feet although still 400 miles from the sea.

In Tibet the Arun has at least two names, the Men Chu for the first 60 miles and then the Phung Chu as far as the Nepal–Tibet boundary. In this paper I propose to simplify the nomenclature and use the name Arun for the whole of the river. The Arun rises on the north slopes of Gosainthan, a peak of the Himalaya 26,291 feet high, and for the first 150 miles its course is mainly east along the strike of the rocks. Heron, who visited the uppermost part of the river as a member of the first Mount Everest expedition, describes how the few abrupt bends that the river makes are such that it avoids areas where granite veins have hardened the rocks. From his geological mapping Heron also showed that in Tibet the Arun tends to flow along synclinal zones. In a region of mature topography rivers usually follow the course of anticlines, since the rocks are there more easily eroded. The fact that the Arun

[1] Formerly regarded as an easterly extension of the Ladak range but now given this name by Burrard.

[2] According to a personal communication from Professor Kenneth Mason the probable error of these maps is not large enough to affect significantly the profiles given in this paper.

[3] *Op. cit.*, Chart XXXVII.



1. The Upper Arun near Pangla, south of Shekar

2. Yo Ri gorge and Kuyok La. Moraine mounds in the middle distance half buried by post-glacial gravels

3. Entrance to Yo Ri gorge

still flows along what were tectonic valleys in the original surface shows how relatively recent is the development of the drainage system.[1]

At present, in its upper part, the Arun is a braded river flowing over an extensive spread of gravels. Harder masses of rock stand up as subsidiary hills within the valley (Plate 1), and where hard bands cross the river minor gorges occur.[2] The valley is still youthful in character, but there has been a recent prolonged period of aggradation which has filled the bottom of the valley with a level spread of gravel. Still more recently the river has begun to cut into the gravels, forming a system of river terraces—features which have struck the eye of all travellers to the region. Although there are many indications of a complex system of terraces which will require more detailed maps and careful levelling for their interpretation, yet the last great spread of gravels and the terraces developed from them can be widely identified even during a hurried journey through the country. Moreover, the age of this conspicuous period of aggradation can be fixed relatively to the period of maximum Quaternary glaciation. Thus a quarter of a mile above the confluence of the Dzakar Chu and the Arun the terminal moraine mounds of a glacier coming down from the Nyonno Ri range are surrounded and almost buried by the gravels of the recent period of aggradation (Plate 2). This moraine is the lowest observed in the district and was formed during the maximum extension of the Quaternary glaciers. Further evidence of age is also found at Rongbuk, where the same spread of gravels merges into the outwash fan of the lowest moraine of the Rongbuk glacier. No fourfold division of the Quaternary glaciation such as is suggested for other parts of the Himalaya by the work of Dainelli, H. de Terra, and T. T. Paterson has yet been recognized in the Arun region. All that can be stated at present is that the spread of gravels was formed at the time of, and after, the maximum Quaternary glaciation of the region. For convenience of reference these gravels will be called the post-glacial gravels and the terraces developed from them the main terraces.

About the middle of its course in Tibet, near Shekar Dzong, the Arun has cut only shallowly (50 feet or less) into the post-glacial gravels. Traced down stream the river is found to have cut increasingly deeper. The Dzakar Chu, a tributary to the south, shows the same features. The heights of the terraces have only been estimated by eye and no reliance must be placed on the absolute values given, but the general increase down stream is obvious. Traced from Tashi Dzom up stream, the terraces of the Dzakar Chu decrease in height above the river, but shortly after Chodzong they begin to increase again, and near Rongbuk they are well over 100 feet. This rather remarkable behaviour of the terraces should be searched for in other streams which turn south towards the crest of the Himalaya. If it is found to be widespread it may be that it is an effect of the recent gradual upwarping of the main Himalayan zone for which evidence is given below.

The largest tributary of the Arun is the Yaru Chu, which rises in the hills

[1] Pascoe, E. H., in the discussion of Heron's paper, *op. cit.*, p. 436, has suggested an alternative reason for this relation between the river courses and the synclines, but it seems probable that Heron's original suggestion is the right one.

[2] Heron, *op. cit.*, p. 420.

north of Kampa Dzong and enters the Arun from the east after passing through the northern continuation of the Nyonno Ri range in the Rongme gorge. Much of the middle part of the Yaru valley has been recently occupied by a shallow lake of which the small lake near Tengkye and the marshes to the south and south-west are the remnants. Spender [1] has stated that he found evidence of this lake east of the village of Sar. Half a mile north of Linga, which lies some 6 miles east of Tengkye Dzong, there is a curious long spur of gravel pushing out southward from the hills on to the flat plain. In 1933 I examined this feature and found that it was a delta of coarse materials put down by a stream when the plain about Linga was covered by a lake some 300 feet deep. The extent of the lake is not known, but should be easily determined when the region is carefully mapped.

The Chiblung Chu, a tributary of the Yaru Chu, cuts through the northern continuation of the Nyonno Ri range at Jikyop in a gorge like that on the Yaru Chu at Rongme. The height of the main terrace in the gorge is some 400 or 500 feet above the river, but traced up stream the height rapidly becomes less, and 7 miles above the gorge is only about 30 feet. Exactly similar changes in the relative height of the main terrace and present river-level are to be seen in the Rongme gorge and have been commented upon by Spender.[2] It would be of great interest to determine with reasonable accuracy the absolute height of the post-glacial gravels as the continuation of the Nyonno Ri range is approached. Without this information the reason for the behaviour of the terraces is in doubt; it may be that the gravels have not been upheaved since deposition, the change in height of the terraces above the river being the effect of rejuvenation which has only worked a small distance to the east of the range. An alternative however, which I believe from the general tectonics of the region to be more probable, is that uplift of the range has caused local upwarping of the recent gravels. Near Sar, also to the east of the Nyonno Ri range, I found fine-grained river or lake deposits tilted away from the range at 20°. A brief examination showed no evidence which would suggest that the high dip was due to land slips, solution effects, or thrusting by glaciers, and the silts seemed too fine-grained to be the fore-set beds of a delta originally deposited at this angle. I had to conclude that the dip was the direct effect of localized earth movement perhaps near a fault. It seems likely that the former lake occupying part of the Yaru Chu valley and the apparently upwarped terraces in the Rongme and Jikyop gorges are the result of recent uplift of the Nyonno Ri range.

The extent of the maximum glaciation in the Upper Arun valley

Before describing the remarkable gorges which occur on the Arun river I wish to review briefly the evidence which seems to me to eliminate the possibility that ice, either as a sheet or as glaciers, has played any important part in their development. All travellers agree that in Tibet evidence of an ice-sheet or of extensive valley glaciers is nowhere conspicuous. A prejudice in favour of the view that Tibet was once covered by ice seems to result from the undoubtedly far greater extent of the Himalayan glaciers in past time.

[1] Spender, M. A., *Geogr. J.* 88 (1936) 293.
[2] Spender, M. A., *op. cit.*

For instance, the Zemu glacier coming from Kangchenjunga formerly extended down to a height of 9000 feet.[1] Had the glaciers running northwards from the Himalayan peaks had a tendency to persist to similar low levels, it is clear that the whole of southern Tibet would have been covered by an ice-sheet; this is the view originally taken by Blandford and later by Trinkler.[2] Odell [3] has tentatively suggested that the Upper Arun drainage area shows some general evidence of a former ice-sheet, but with this I am not able to agree. Hayden stated that moraine from the mountains to the south occurs on the hills near Kampa Dzong. This however might be explained by a single glacier tongue from the Pauhunri group, though I observed neither near Kampa Dzong nor in the Kanchenjhau and Pauhunri region anything which would suggest this. An extension of the glaciers to far lower levels in the Himalaya than in Tibet is to be expected because of the steeper gradients, and because of the far higher precipitation. Even during a secular lowering of temperature such as would have no doubt occurred during the Glacial Period it is probable that Tibet would require a considerable increase in precipitation to nourish an ice-cap. In Pleistocene times the monsoon may have been a little stronger and the Himalaya a little lower, yet I believe that the precipitation reaching Tibet was still too small to allow an ice-cap to form.

Within the Arun drainage area I observed no definite traces of the effects of ice away from the higher summit of the Nepal–Tibet watershed and away from the Himalaya; there were on moraines, no "roche moutonnée" or striated surfaces, no "crag and tail" effects, no U-shaped valleys or glacially truncated spurs. It has been suggested that Tibet would support an ice-sheet which would be so nearly stagnant that the usual effects of moving ice would not be produced. Such a view is difficult to disprove. However gradients in much of England where the effects of the Quaternary glaciation are conspicuous are much less than in this part of Tibet, and small hills, as for instance those which rise up in the valley of the Phung Chu near Pang La (Plate 1), should not have remained unmodified by the ice had it ever covered the region. The waning stages of an ice-sheet as it broke into lobes occupying the lower valleys should in particular be indicated now by terminals and lateral moraines, but none have yet been found. The present glaciers flowing north from the Himalaya all show evidence of being more extensive in the past, but the amount of extension was small. The lowest moraine of the Rongbuk glacier is at Rongbuk, only 5 miles from its present snout. The extension of other glaciers in the Nyonno Ri range, the Lashar Plain valley, the Lhonak valley and about Pauhunri is comparable with the extension of the Rongbuk glacier. Thus in this part of Tibet there only seems to be definite evidence of a slightly greater Quaternary extension of the present glaciers and no evidence for the former existence of an ice-sheet.[4]

[1] Garwood, E. J., Appendix, pp. 298–9, to 'Round Kangchenjunga,' D. W. Freshfield, London, 1903.

[2] *Geogr. J.* 75 (1930) 225–32.

[3] *Geogr. J.* 66 (1925) 311–13.

[4] De Terra, from his travels in Little Tibet, has come to a similar conclusion. *Geographical Review*, vol. XXIV, 1934, p. 32.

The Yo Ri gorge

Yo Ri, a mountain 17,987 feet high, dominates on one side the remarkable uppermost gorge of the Arun, while on the other it overlooks a low pass, the Kuyok La, crossed by the mule track up the Arun valley as no way is possible through the gorge. The present configuration of the region is remarkable because the river has cut a deep, artificial-looking gorge like a railway cutting between high mountains, whereas the way by what is now the Kuyok La would seem to have offered a much easier route. Twelve miles above the gorge, where the Arun is joined by the Yaru Chu, the river is flowing in a broad valley over the post-glacial spread of gravels. Traced down stream the valley remains open, but the river cuts gradually deeper into the gravels lying in the bottom of the valley until at Kharkung, immediately before the gorge, the river is flowing 700 or 1000 feet below the upper surface of the gravels. Here the river plunges into the mountain side (Plate 3). The pass which the river avoids is only about 1800 feet above the river, and is broad, with slopes of easy gradient. The steep walls of the gorge tower 6000 feet above the river on the west side and still more on the east. The gorge runs south for 4 miles then turns through a right angle and continues in that direction for another 4 miles before ending as suddenly as it began in a broad valley. This valley is also floored by post-glacial gravels belonging to a set extending up the Dzakar Chu, a tributary of the Arun. Into these gravels the rivers have cut 100 feet or so (Plate 2).

The Yo Ri gorge was briefly described by Heron in a paper to the Society,[1] and it was singled out for discussion after the paper by Mr. C. S. Fox and Mr. A. R. Hinks, who both realized its extraordinary interest. No satisfactory explanation of it however was reached. When actually confronted with the gorge it is impossible not to feel that, if this apparently unreasonable behaviour of the river could be understood, it would throw light on the cause of the anomalies of the whole Arun drainage.

Heron observed that the Yo Ri gorge is cut in hard gneiss while the pass to the east is cut out of comparatively soft schists. I believe that the germ of the explanation of the gorge lies in this observation which was confirmed in 1933. The change from the relatively soft schists of the Kuyok La to the hard resistant gneisses is abrupt. Some shattering of the harder gneiss, probably produced by faulting, was noticed at the entrance to the gorge. This has not the appearance of the usual Tertiary gneisses, but seems rather to belong to what I have tentatively called Peninsular India type.[2] Whether faulting or an unconformity has produced the abrupt change in the rocks was not decided.

Had the Arun ever flowed over the Kuyok La in relatively soft rock it would never have changed by any ordinary process of river capture to its present course through the corner of a mountain mass of much greater height and composed of harder rock. On a small scale ice-sheets have produced channels across spurs, but not only is the Yo Ri gorge on a very different scale but there is no evidence in the region of a former ice-sheet. The enormous thickness of sediments eroded from the region must be taken into considera-

[1] *Geogr. J.* 59 (1922) 421, and plates opposite pp. 420 and 421.
[2] 'Everest 1933,' p. 320.

4. River Arun immediately above the main gorge

5. Looking down the main gorge of the Arun

6. Looking north at part of the Great Himalaya Range

Houston–Mount Everest Flight.
By permission of "The Times"

tion in any explanation of the present gorge. Above the present surface there formerly existed the Mount Everest Limestone Series, the Lachi Series, and the Jurassic and Cretaceous beds, in all many thousands of feet. The Arun once flowed on a surface high above the present one, and the course it now follows may be taken as giving an indication of the form of this surface since the river must have followed the easiest downhill route. In this surface the river incised itself and formed a valley; at the same time general erosion by rain and streams lowered the whole area. The Arun, in its upper part, is still cutting into soft sedimentary beds, and as a result the valley is wide with gently sloping sides. Where the Yo Ri gorge occurs, the river, in cutting down, has come across hard gneisses. The gorge which has been produced, as in many other cases, is simply the result of the change to harder rocks. Rain, streams, and the other erosive agents which tend to widen valleys have had far less effect on the hard gneisses of Yo Ri than on the softer sediments farther up stream, and thus the valley sides remain steep. The change from the two types of rock occurs abruptly. The Kuyok La marks the beginning of the softer rocks and has been produced by the general erosion of rain and small streams and not by the river.

It is possible to decide what causes the change in the hardness of the rocks to which the formation of the gorge is due. The rocks forming the central core of the Nyonno Ri range were reached at Yo Ri and on the east side of the range above Phuru. They proved to be gneisses of Peninsular India type which underlie the schists and gneisses of the Mount Everest Pelitic Series and Limestone Series. The Nyonno Ri range is therefore structurally an anticline which brings up deep-seated and harder rocks. The deflections in the courses of the Arun and the Yaru Chu round the Nyonno Ri range suggest that an early stage of this anticline existed as an upward bulge of the surface on which the drainage was established. The increase in height of the main terraces above the river as the Yo Ri gorge is approached suggests [1] that uplift is still going on (*cf.* below). As uplift and denudation continue it is likely that the Yaru Chu where it makes an abrupt elbow bend at Sar will one day find itself cutting into the harder gneisses below the sediments on which it is now flowing and then another gorge very like that at Yo Ri will be formed.

The most interesting feature of the Yo Ri gorge is its close association with the low pass of the Kuyok La. If the original surface on which the Arun flowed had been anything like the present one then the river would have taken a course over the lower ground to the west of Yo Ri which is now the Kuyok La. Since this is not the case the Arun must have firmly established its course before the development of the present form of the land. Some of the present greater relative height of the Nyonno Ri range above that of the land to the north-west and east is to be ascribed to the direct effect of the anticlinal uplift, but much is probably the result of a secondary effect, namely, the resistance to erosion of the hard gneisses of Peninsular India type which form the range. Subjected to roughly the same intensity of erosion the hard

[1] This can only be suggested. The possibility that it is due to rejuvenation cannot be ruled out until accurate measurements of the absolute height of the terraces have been made.

rocks of the Nyonno Ri range are lowered much less than the softer rocks to the north-west, and thus is produced gradually a greater difference in height than is produced by the direct effect of uplift. The history of the development of the Yo Ri gorge, as I read it, implies that the Arun has had exactly its present course for a very long time, long enough in fact for the great inequalities in height between the Nyonno Ri range and the ground to the west to be developed.

Minor gorges on the Yaru Chu and Chiblung Chu

The Yaru Chu crosses the continuation of the Nyonno Ri range by the Rongme gorge already mentioned, and its tributary, the Chiblung Chu, crosses the same range a few miles to the north by the similar Jikyop gorge. To the east of the range there is a low col between the two river valleys which is 2000 feet lower than the range itself. Heron [1] commented upon this and suggested that the Yaru Chu once flowed over the low col and joined the Chiblung Chu. He supposes that a stream has since cut back along the present river gorge at Rongme and captured the Yaru Chu. An explanation essentially the same as given for the Yo Ri gorge seems however more satisfactory. The rocks at the col are almost unmetamorphosed and easily eroded sediments, while those of the range have been hardened by metamorphism which was sufficiently intense for biotite to have developed in the slates. General denudation must have been more effective over the region to the east of the range and lower ground is therefore to be expected there. Further, the height of the river terraces and the existence of the former Yaru lake suggest that there has been localized uplift of the Nyonno Ri range. This would also have the effect of producing relatively low ground on the east side. Besides the direct effect, the greater uplift has exposed more deep-seated rocks which, being harder, are less easily eroded. The difference in the hardness of the rocks is the factor essentially responsible for the gorge form, but it is itself the result of a localized region of special uplift.

The main Arun gorge and its origin

For 11 miles below the Yo Ri gorge the Arun river flows in a moderately wide valley floored by the post-glacial gravels. The river is at present cutting into the gravels and the terraces lie 100–200 feet above the river. The main Arun gorge begins abruptly just below the confluence of the Kharta Chu. From a spur 1000 feet above the river at the entrance to the gorge a traveller may look north over a treeless Tibetan landscape with the river flowing between high gravel terraces (Plate 4). Looking south from the same point the view is typically Himalayan (Plate 5); the valley is a narrow wooded defile with such steep sides that it cannot be followed by a mule track. The first description of this gorge was given by members of the Mount Everest reconnaissance expedition [2] and it was explored by Morris and Noel, members of the second Mount Everest expedition.[3]

From the junction with the Kharta Chu to Kimathangka the river descends

[1] *Op. cit.*, p. 421.
[2] 'Mount Everest; the Reconnaissance 1921,' pp. 110–11 and pp. 125–6.
[3] 'Assault on Mount Everest 1922,' pp. 89–102.

3300 feet in 18 miles, and the descent is made without definite waterfalls. The village of Lungto near Kimathangka is stated to be situated on a terrace or bench 1200 feet above the river,[1] and indeed traces of similar high terraces are to be seen at the top end of the gorge. Below this point the river enters Nepalese territory and Europeans have not been allowed to visit it, but from the maps it is clear that the river continues to flow in a gorge for another 40 miles, by which time it has descended to a height of only 2000 feet. This part of the valley can be roughly followed by a track while villages and cultivated areas occur at intervals.

The main gorge of the Arun is a magnificent example of a feature which occurs on many of the Himalayan rivers. The neighbouring Tista valley, being in Sikkim, is better known. The part of this river which is Tibetan in character is short, then the valley narrows and becomes a gorge of the same type as that of the Arun but not so impassable. This is no doubt because the river is smaller and is not able to cut downwards so fast as the Arun. The profile of the Tista (see folding map) is closely comparable with that of the Arun, and again the gorge corresponds to the lower half of the convex portion of the profile. Between Singhik and Dikchu the valley widens and some lateral erosion by the river begins. In this region the profile changes from being convex to concave again.

The profile of Himalayan streams and rivers will provide an interesting study when accurate data can be obtained. Even from the existing maps of Sikkim profiles can be drawn which show certain significant features. Thus streams of the foothills give an approximation to the even curve of a graded river. Streams which rise among the once glaciated hills from 12,000–15,000 feet high have this even curve interrupted. In the case of two valleys near the Natu La, which were visited during the 1933 Mount Everest expedition, the change from the normal curve of water erosion takes place at the lowest point of the former glaciers of the district. Here, where the ice had practically no forward motion and where masses of moraine were being put down, the valley lies above the level which would be expected if the curve of water erosion was continued to the watershed. At this point there is also a change from the narrow V-shaped valley, due to river erosion, to a broad upland valley with U-shaped cross-section. The nature and extent of glacial erosion are variously assessed by different observers and this is not the place to consider the problem, but I am convinced that useful conclusions would come from a careful study of the region about the Natu La.

The unusual profiles of the Arun and Tista, their gorges, and their peculiarity in cutting through a high range, seem to be related facts having the same cause. The views of different observers on the origin of the gorges are summarized by Hayden and Burrard in 'A sketch of the geography and geology of the Himalaya Mountains and Tibet,' which was revised in 1934 by Burrard and Heron. Despite difference in detail the various theories are essentially of only two kinds, those based on the idea of antecedent drainage and those based on the idea of river capture. In the revised monograph the Arun and Tista are still, without question, given as examples of rivers which have cut back their headwaters and captured Tibetan streams. The number

[1] 'Mount Everest; the Reconnaissance,' p. 125.

of factors controlling the behaviour of rivers in such a region as the Himalaya makes it impossible to reach a decision between the alternative theories by any deductive reasoning. It happens however that in the case of the Arun the existence of the remarkable Yo Ri gorge provides definite evidence in favour of antecedent drainage. The peculiarities of the topography about Yo Ri can only be explained on the view that the present course of the Arun was established a long time ago on a surface high above the present one, and that since that time some localized uplift and much general erosion has greatly modified the original form of the surface. The possibility of any recent capture of the upper part of the present Arun drainage by a Himalayan river seems therefore to be eliminated. The hypothesis of antecedent drainage also provides a reasonable explanation of the wide departure in the profile of the Arun from that of a graded river. The present profile of the Arun must be regarded as a temporary stage due to uplift of the Himalayan part of the valley along with the general uplift of the whole Himalayan region. The thick gravels flooring the valley were the result of a period of aggradation which extended into post-glacial time. It seems likely that this aggradation is to be ascribed to more vigorous uplift which partly dammed back the river or at any rate reduced its gradient. The original Arun must have found a downhill course among the minor undulations of the original mountain slope which formed the descent from the Tibetan plateau to the Gangetic plain, in those days perhaps an arm of the sea. No such general surface exists nowadays because since that time the range of the Himalaya has risen across the middle part of the river's course.

Isostasy and the upwarping of the Himalayan region

The effect of loading and unloading the crust of the Earth can be studied in the records of the formation and dissipation of the Quaternary ice-sheets of the world. Despite complicating factors, due for instance to changes in the volume of water in the oceans or to mountain building movements in the crust, observations in Scandinavia and North America demonstrate conclusively that when thick masses of ice have developed the crust has been depressed, and that later when the ice has disappeared the crust has slowly and somewhat irregularly risen. The difficulty of understanding the mechanism of these effects, the obscurity of their relations to the complex results obtained by gravity surveys, and the confusion that exists about the exact meaning of the word isostasy, should not be allowed to mask the fact that the loading and unloading of the Earth's crust has the effect of raising and lowering the average altitude of the surface by an amount which is roughly proportional to the load added or subtracted.

Where the Arun crosses the Himalaya the major topographic units are less complex than elsewhere in the range (Plate 6); the Tibetan plateau is a fairly even surface at an average height of about 16,000 feet, and the Himalaya closely approaches a linear feature. As a result of this simplicity certain probable effects of erosion can be better discussed for this region than for other parts of the Himalaya. The removal by the rivers of vast quantities of rock to form the deep valleys which here cross the range should have produced local uplift due to reduction of the load on the crust just as uplift has taken place

with the disappearance of an ice-sheet. Nansen[1] and Jeffreys[2] have both discussed from a theoretical point of view the nature and extent of the vertical uplift to be expected where valleys are formed and deepened in a mountain range. The Arun river provides evidence that the effects which Nansen and Jeffreys considered theoretically likely have in this part of the Himalaya actually taken place.[3]

Many observers have given evidence of recent and even present uplift of the Himalaya. Pliocene overthrusting in the foothills was proved by C. S. Middlemiss,[4] who also showed that the recent Karewa deposits sweep over the Pir Panjal range with dips which in places reach 50°.[5] The latter observations have been confirmed by Dainelli.[6] De Terra[7] has recently given geological evidence that the uppermost topographic relief in the area between Kashmir and the Indus valley "has suffered from gentle warping, long after the Tertiary orogeny, and subsequent even to the intra-Pleistocene folding." Some of the recent earth movement in certain parts of the Himalaya seems to be the result of a continuation of horizontal pressure which produced the original region of high elevation, but some seems to be vertical uplift of a different sort. In Sikkim and the neighbouring parts of the Himalaya there is evidence that uplift to maintain approximate isostatic equilibrium during the development of the valleys is the cause of the upwarping and is responsible for the extra height of the Himalayan range above the plateau of Tibet. I have briefly stated elsewhere[8] that if the Sikkim part of the Himalaya were levelled by piling the tops of the mountains into the adjacent valleys, a plateau sloping gently towards the plain of the Ganges would result, and that the height of this plateau where the main range of the Himalaya now stands would be approximately 15,500 feet. It is significant that 15,500 feet is a little lower than the average height of that part of the plateau of Tibet which lies immediately to the north of Sikkim. In relation therefore to the isostatic balance of the crust the Himalayan range is here equivalent to the southward extension at a steadily declining height of the edge of the Tibetan plateau. It has been shown above that the Arun and Tista rivers must have originated on just such a surface. Here then is some evidence that maintenance of approximate isostatic balance during a period of erosion is the cause of the upwarping. The Eastern Himalaya as I visualize it have been produced in two distinct stages[9] and by two different processes. The first stage was the production by horizontal compression of an elevated plateau in approximate

[1] Nansen, F., "The Earth's crust, its surface-forms and isostatic adjustment." Norske Videnskaps-Akademi, i Oslo i Mat. *Natur.*, Klasse 1927, No. 12, 1928, pp. 1–21.

[2] Jeffreys, H., Gerlands Beitrage zur Geophysik Bd. XVIII Heft, 1/2, 1927, pp. 17–18.

[3] Garwood, in D. W. Freshfield's 'Round Kangchenjunga,' has given other evidence for vertical uplift and tentatively suggested that isostatic uplift consequent on shrinkage of the glaciers of the region was the cause.

[4] *Mem. Geol. Surv. India*, vol. XXIV, Pt. 2, pp. 59–142, 1891.

[5] *Mem. Geol. Surv. India*, vol. XLIV, p. 241, 1923.

[6] See E. G. Garwood's review. *Geogr. J.* 63 (1924) 243–6.

[7] H. de Terra, *Mem. Connecticut Acad. Surveys*, vol. VIII, p. 67.

[8] *Nature*, vol. 132, p. 28, 1933; and 'Everest 1933,' pp. 328–31.

[9] This conclusion I have stated before ('Everest 1933,' pp. 330–31). De Terra has recently given other evidence for the same view (*op. cit.*, p. 71.)

isostatic equilibrium. This plateau is essentially the present plateau of Tibet, but when formed it extended farther south over the region which is now the Himalaya. The second stage, which is presumably still going on, is one of vertical upwarping of the edge of the plateau to maintain approximate isostatic balance as the rivers cut deeper and deeper valleys into its edge. The second stage is the one which has produced the extra height of the Himalayan mountains above the plateau of Tibet. In the Eastern Himalaya, without the deep valleys and gorges, it is probable that there would have been no peaks towering 10,000 feet above the plateau of Tibet.

THE BRITISH GRAHAM LAND EXPEDITION, 1934-37

SINCE the work of the British Graham Land Expedition was last summarized in the August number of the *Journal*, a whole season's sledging has shown that the lower third of the map published with that summary must be radically altered. Before the flight of Sir Hubert Wilkins in December 1928, Graham Land was thought to be a long peninsula of the main continent. The indications of the photographs and observations from that flight were that the peninsula was an archipelago with at least three channels cutting through it, which were named Crane Channel, Casey Channel, and Stefansson Strait, from north to south respectively. The existence of the two southern channels is now disproved, and much doubt is cast on that of Crane Channel. The work of one Australian, Rymill, has undone the work of another, and it must be admitted that photographs from the air under the conditions of a long and hazardous flight may lead to entirely erroneous conclusions. Until the expedition returns with its detailed surveys it is impossible to estimate how great the changes in the map will be, but it seems likely that Stefansson Strait will have to be replaced by a fjord opening eastwards from a more or less ice-covered plateau up to 7000 feet in height, and that Alexander I Land must be magnified to at least five times its supposed size.

It will be remembered that the *Penola* left the shore party on Barry Island, at the north-eastern corner of Marguerite Bay, in March 1936. Rymill had with him at the base Bertram, Bingham, Fleming, Hampton, Meiklejohn, Moore, Riley, and Stephenson. The early winter (southern) of 1936 was spent in training the dog-teams for the projected sledge journeys, overhauling the equipment and fitting skis, instead of floats, to the plane. Blizzards at the beginning of May, the worst yet experienced by the expedition, delayed the formation of the sea-ice; the wind at its strongest reached a force of 110 m.p.h., and a hurricane force of 90 m.p.h. was recorded for several days, accompanied by low temperatures. By May 15 however the ice was bearing in most places, after a period of calm, cold weather, and nearly a month later it had reached a thickness of over $1\frac{1}{2}$ feet. Conditions seemed to promise well for the spring sledging programme.

On June 9, in the uncertain winter twilight, Hampton, Rymill, and Fleming made a flight southwards after two trial trips, and saw, from a height of

3000 feet, a considerable distance south and west over Marguerite Bay. No open leads were seen. On June 11 the first depot-laying party, Rymill and Stephenson, started from the base with their dog-teams, followed the next day by Bertram and Bingham with two teams, and by Hampton and Riley in charge of the tractor towing a load of 1600 lbs. Sledging conditions seemed propitious, the sea-ice had been bearing for nearly a month, and they planned to travel southwards for about 60 miles to establish a depot for use on the projected sledge journeys. The three units combined and proceeded together for the next four days, during which they made only 25 miles over a heavy snow surface, during the brief hours of twilight.

On June 17 there was a violent blizzard, accompanied by a rise in temperature. Camp was pitched on the sea-ice, which about midnight broke up all round the tents, though fortunately not between or under them. Next morning the depot and tractor were left behind until conditions should improve, while the men made a difficult journey to land over huge pressure ridges and slush from floe to floe, often with the aid of an electric torch. They were held up at their camp on Terra Firma Island until June 25, and reached the base two days later after a laborious journey.

On July 19 two independent parties left the base to make a topographical and geological survey of the immense fjord system north of the base. Fleming, Riley, and Stephenson were at work along a number of branching fjords, of which the largest was 35 miles long; Rymill and Bertram continued the survey northwards by compass and made their way up Laubeuf Fjord, the strait separating Adelaide Island from the mainland. Both parties returned to the base early in August, having, between them, surveyed the complex coast-line for 70 miles north of the base, which involved mapping about 250 miles of unknown coast-line.

In the middle of August Bingham and Fleming started south to locate the depot which had been left 35 miles from the base in the vicinity of Terra Firma Island, on which the midwinter depot party had sought refuge from the shifting ice six weeks earlier. They took four days to reach the island, were then delayed by bad weather, and were obliged to feed the dogs on half rations until Hampton and Rymill flew down with further supplies. The whole journey took a fortnight. At the same time Riley and Bertram went north to collect seals from a cache on an island 30 miles away. They found that the sea-ice had never been broken since it was first formed in May and, travelling swiftly on a firm surface, they were able to complete their journey in three days in spite of a heavy load of seal meat. The varying fortunes of these two sledge parties, with about the same distance to travel, show only too well the changeable conditions.

Three flights, yielding valuable information, were made on August 15, August 17, and September 4. On August 15 Hampton and Rymill flew westwards over Marguerite Bay, following the northern shores of Alexander I Land until they came in sight of Rothschild Island, 150 miles from the base, and turned for home, as the plane had a range of only 300 miles. Strip photographs were taken of the entire coast. During the first part of the flight they were above unbroken ice covering Marguerite Bay; but off Alexander I Land there were leads of open water and the ice itself was broken and rough. Inland,

high rocky mountains rose through a thick mantle of glacial ice, which flowed as a smooth stream towards the sea, to terminate in a fringe of blue ice-cliffs broken only at one point where a rocky spur comes down to the shore. The fliers were also able to obtain an excellent though distant view of a strait or fjord in the south-eastern corner of Marguerite Bay. Hampton and Stephenson had seen this feature in March 1936, but from the better angle at which Hampton and Rymill saw it they realized that it extended farther south than had previously been supposed. On August 17 Hampton and Stephenson flew south to try to investigate this feature, but not until September 4 was there a day clear enough. On this occasion they were able to fly 140 miles to the south, to lat. 70° 05′ S., 40 miles down a passage from 5 to 10 miles wide.

The first of these flights had shown that a sledge party could not expect to approach the western shores of the Dependency by skirting the northern coast of Alexander I Land. The sea-ice is treacherous off the headland, and there are no off-lying islands to afford safe camp sites, while a coastal fringe of high ice-cliffs provides an effectual barrier to sledging on the land glaciers. The last flight had opened up a more promising route, by sledging southward through the low passage over which the plane had flown and then turning westwards. In accordance with these indications therefore Stephenson, Fleming, and Bertram left the base on September 5, supported by Bingham and Rymill as far as the depot about 90 miles south of winter quarters. From here they climbed a glacier to a saddle and descended into the curious strait or channel which had been seen from the air, and which turned out to be 200 miles long and about 15 miles wide, flanked on either side by high land. They sledged down this strange channel, which was covered with barrier ice throughout its length, until they had reached lat. 72° S., long. 67° W., at which point it seemed to be expanding into a large bay. On the west was the continuous coast of a much-enlarged Alexander I Land, while on the east was Graham Land. Both sides are high and mountainous, and should have been well surveyed, unless the persistent fog limited such activities. The origin of such a long, narrow channel of so unusual proportions may well be explained when the geology is written up, since Fleming found that on the Graham Land side there were the ancient eruptive and metamorphic rocks already seen further north, but on Alexander I Land there were sedimentaries with both vertebrate and plant fossils. It must therefore be a fault structure, but it is situated where geologists expected folds.

The party returned up the western side of the strait and, when 80 miles from the base, met Rymill and Bingham on their way out for a long journey to the south-east. They left their last depot 100 miles south of the base, and from that point began a long ascent into the interior, relaying their loads through deep snow, until they reached a pass through the mountains at a height of 7500 feet, about 90 miles from the depot. This was the highest point reached, and from there they descended into broken and much-crevassed country, over which they made slow, tortuous, and often dangerous progress, as far as a point overlooking the east coast in lat. 70° 40′. Here two days were spent making geological collections and surveying, after which they proceeded north. The weather deteriorated, and at one point they were forced to spend seven successive days in their tent. During the return journey conditions

were better, and they covered the first 100 miles in four days. On reaching the west coast again the 60 miles of sea-ice between them and the base was found to be in process of breaking up, but these conditions had been anticipated and depots had been left on various islands.

When they returned after ten weeks' absence on January 5 they had covered 535 miles, had extended the survey across Graham Land in lat. 69° 50′ S., and had mapped 140 miles of its east coast as far south as lat. 70° 40′ S. No one had previously set foot on the east coast of Graham Land south of 66° 30′ S., 300 miles farther north. In this latitude a plateau rises gradually from the west coast to 7000 feet, east of which the country is broken up into mountain ranges, with many summits over 8000 feet, and small, terraced plateaux lying at different levels between them. The east coast runs north and south and lies 30 miles east of its previously supposed position. The coast is clearly defined by crevassed ice-slopes spilling into the shelf-ice of the Weddell Sea. During most of the thirty days the party spent surveying this coast fog enveloped the shelf-ice. With easterly winds the fog crept up the bays, the large glaciers giving a deceptive appearance of broad channels running westwards between islands. Some such conditions as these no doubt convinced Wilkins that there were straits where, in fact, the land is some thousands of feet above sea-level. The hopes that this party would sledge far down into the Weddell Sea were frustrated by the arduous climb over the mountains; but the geographical results are nevertheless of unusual interest.

Meanwhile the ship's party, having spent the Antarctic winter of 1936 at Port Stanley, left for South Georgia in August to refit at Stromness, where facilities had been put at their disposal by the kindness of the Vestfold Whaling Co. through the good offices of Mr. E. D. Naess. The refit was completed successfully, after which the ship returned to Port Stanley, leaving there for the south to pick up the advance party on 29 December 1936. The *Penola* planned to reach the old base, in the Argentine Islands, some time in the middle of January 1937, and wait there until ice conditions permit the voyage to Barry Island. If conditions prove favourable, it is expected that the whole expedition will have left Barry Island about the middle of February, reaching Port Stanley before the end of March.

It was arranged that Roberts, one of the biologists of the expedition, should be left behind at South Georgia when the ship returned to Port Stanley, to carry out a month's scientific work. He made his headquarters at Grytviken, where he had the use of the laboratory belonging to the *Discovery* Committee. He returned to the Falkland Islands on board R.R.S. *Discovery II* at the end of November. F. D.

This summary, principally of wireless despatches published in "The Times," makes it clear that Mr. Rymill's expedition has obtained very important geographical results, upon which they are to be congratulated. We must await their surveys before attempting to relate their discoveries to the photographs made by Mr. Ellsworth on his flights of 21 and 23 November 1935, published in this number of the "Journal."—ED. *G.J.*

A NOTE ON THE OXFORD UNIVERSITY ELLESMERE LAND EXPEDITION, 1934

Dr. Noel Humphreys writes: I should like to add something to the account given in the May 1936 *Journal* of the crossing of Ellesmere Land by one of the sledge parties. A further note seems necessary, as a footnote added to my paper put forward a theory that the crossing was to Troll Fjord, not Vendom Fjord as suggested by me in my paper. The theory in the footnote is not of course opposed to my statement in the paper, that we must have reached Vendom Fjord provided Sverdrup's map is correct. My companions and I were in agreement that we had travelled south-east from our camp near the Gretha Islands, and Vendom is the only fjord shown in that direction in the Sverdrup map. The theory advanced in the footnote rests on the assumption that the Sverdrup map is inaccurate, and that Troll Fjord also lies south-east of the Gretha Islands, instead of just west of south, as shown on the map.

I raised this point with others of the expedition immediately following our return from the sledge trip to our base at Etah, and the conclusion reached was based on three reasons: (1) I produced times and bearings from my diary and Sergeant Stallworthy then independently estimated from the times, the sledge loading, the type of country passed through, and the conditions of the snow that we ought to have covered 71 miles during the crossing. This distance differs by only 2 miles from the distance which with windings I myself reckoned to have covered to Vendom Fjord according to Sverdrup's map. The distance in a straight line according to the map is 54 miles to Vendom Fjord, 16 to Troll Fjord. (2) It was suggested that the map was incorrect, and that we had reached Troll Fjord. Sverdrup's book 'New Land' was consulted, and it was found that two series of sun observations had been made by his party at Troll Fjord, which therefore made it unlikely that the map is seriously incorrect at this point. (3) We also carefully read the description of Troll Fjord, and it differed in every particular from the fjord of our seafall. Sverdrup's party found it impossible to penetrate inland from the head of Troll Fjord, whereas the fjord we reached ended as a broad alluvial plain just above sea-level, and it was possible to sledge inland at any point. Troll Fjord was also described as having "high threatening walls of rock on both sides" near its head, where it was "as narrow as a gut," and the fjord ended "for all intents and purposes in a steep cliff." Troll Fjord moreover had at its head a valley running in from the east, ending 2 miles inland in "mountain walls, high and black." All these features were absent at the head of the fjord we reached.

The conclusion therefore formed from our discussion at Etah was that the fjord we had reached could not in any case be Troll Fjord and that, without any reasonable doubt, we had reached the head of Vendom Fjord.

PEARY'S JOURNEY TO THE POLE

PEARY. By WILLIAM HERBERT HOBBS. *New York [and London]: The Macmillan Company*, 1936. $9\frac{1}{2} \times 6$ *inches;* xvi+502 *pages; illustrations and maps.* $5 [25s]

PROFESSOR HOBBS has done well to bring together in an interesting book the long story of Peary's explorations in the Arctic from 1886, when he first travelled on the ice-cap in Greenland, to 1909, when he came back from his North Pole expedition to get a very mixed reception from his fellow-countrymen, of whom many took the side of a rival claimant for the honour of being first at the Pole. The extent of the feeling against Peary in his own land, and the remarkable ways in which it was manifested, had, one might have hoped, been regretted by all concerned and forgotten by the rest of the world. It is perhaps surprising that his biographer should now have given four whole chapters to relate how, after Peary had sent an unwisely worded telegram from Indian Harbour on his way home in 1909, the partisans of Cook in the press, on the platform, and in Congress, successfully spoiled Peary's legitimate satisfaction in his achievement. This lamentable affair deranges the latter chapters of the story; it will be more profitable to examine how Professor Hobbs treats the gradual development of that technique of polar exploration for the single purpose of getting to a defined point and back over moving pack, which is often quoted as the "Peary system," but is not always well explained.

Admiral Peary was not especially good at giving an orderly account of his journeys, and this may perhaps have delayed a proper appreciation of his merits. He published the story of his last and greatest journey serially in the United States in *Hampton's Magazine* for Jan.–Sept. 1910 and in *Nash's Magazine* for the British public in Feb.–Sept. 1910; these are now hard to find. The story is said by Hobbs to be written with great care. 'The North Pole,' published in September 1910, "is a different narrative account, since it covers 373 pages." It was planned or written "in hotels, on trains and street cars and taxicabs even," largely during his tour of Europe in the spring and summer of 1910, and "for a careful reading of the narrative of the sledge journey both accounts are necessary to fill all the gaps in details." Professor Hobbs therefore uses both, and the personal narratives of Bartlett, MacMillan, Borup, and Henson. Writing at leisure after many years he has been able to make his own description more systematic, but on the most important matter, the often-quoted System, he seems to us to be a good deal less clear than Peary himself, who on pp. 201 *et seq.* of 'The North Pole' (General Hubbard edition) gives the main points of it: they include "To have a sufficient number of divisions, or relay parties, each under the leadership of a competent assistant, to send back at appropriate and carefully calculated stages along the upward journey" and "To return by the same route . . . using the beaten trail and the already constructed igloos to save time and strength"; the latter is emphasized by italics. Each party as it returned would "knit together" the breaks in the trail caused by movement of the ice, and thus keep it open for the speedy return of the "main party," *i.e.* in the end the small Polar party. Peary started with six of these divisions, in charge respectively of Bartlett, Marvin, Borup, MacMillan, Goodsell, and himself; Henson was sometimes detached but was never counted as a division leader; there were 17 Eskimos, 19 sledges, and 133 dogs.

One division, generally Bartlett's, pioneered the advance, a march or so

ahead. The remaining divisions marched more or less together, Peary's in the rear, until one by one they were sent back with enough stores to take them home at double the speed of coming out. Before each division was sent back under its white leader it had its best Eskimo drivers and dogs taken from it, and the worst given to it, so that selection was always in operation, a most important element of the System. It was intended that one division should turn back at the end of each five days, and in spite of delays and casualties this worked out very nearly to plan:

They started on February 28 and March 1, and were held up about March 4–10 by a wide lead. Goodsell turned back March 14; MacMillan on March 15; Borup on March 20; Marvin on March 26; Bartlett on April 1; Peary reached the Pole on April 6 and turned back on April 7.

This successful strategic plan is very clearly set out in Hobbs' diagram facing p. 344 but confusingly described by him on p. 333, where he says that the plan "was to comprise 5 co-operating independent expeditions, four of which were supporting expeditions, which had for their primary object to place the commander of the main party at an ultimate advanced base upon the sea ice. . . ." To describe Peary's organization of six "divisions," one pioneering and five a march behind, as five independent expeditions does little to assist the student of polar strategy.

Peary's great merits as a traveller have been so much obscured by controversy that it has seemed well to examine in some detail the system which made it possible for him to reach the Pole, and which explains very naturally the superior speed at which he was able to travel on the final marches when he had taken the cream off and got out of the way the "supporting" parties in front of him, or when on the return he had only to "come galumphing back" on a well-trodden route. It is the more regrettable that we have not yet the material to study the performance in full detail. Professor Hobbs thanks the family of the explorer "for placing documentary material at his disposal," but he says little or nothing about what is in the original diaries and records that few, as we understand it, have seen. If one may judge from what has been published, the choice of material and the way it has been discussed have not done it justice. 'The North Pole,' in the General Hubbard edition of 1910, reproduces tracings from the original records and computations of five sights: 2 by Marvin and 1 by Bartlett on the march northwards, and 2 by Peary (those we have numbered later 1 and 4) at Camp Jesup near the Pole. All these are worked up as if they were meridian observations for latitude, as indeed they evidently were, more or less: but they are much better if without any such assumption they can be treated as sights giving position lines. Professor Hobbs does nothing to elucidate this matter, and if we now examine it with a particularity rather unusual in a review, it is because we believe that neither his biographer nor Peary himself has succeeded in doing plain justice to his observations.

In the narrative (p. 357) Hobbs says: "The first sun observation at noon, with such corrections only as were possible at the time, made the latitude of this camp 89° 57′, three miles only from the Pole itself. As later corrected . . . 89° 55′ 22″.24." He does not remark that the first single observation gave not a latitude but only a position line; and that a corrected position given to a hundredth of a second of arc is open much more widely than Cook's 89° 59′ 45″ (p. 413) to his own comment on the latter, that "every Polar explorer should know that it is quite impossible to fix the polar position astronomically with any such accuracy."

He relates later how the National Geographic Society appointed a Sub-Committee on Research, to whom the original journal and records were

submitted, and how on 4 November 1909 this sub-committee reported their unanimous opinion that Peary reached the North Pole on 6 April 1909 (p. 388). He tells how Mr. E. A. Reeves "requested and obtained from Peary a copy of his observations," and after careful recomputation concluded "that an uncertainty of about ten miles was all that could reasonably be allowed for the distance of Camp Jesup from the Pole" (p. 399). He says further, in a footnote on p. 400, that Professor Hudson B. Hastings made an unofficial but very careful recomputation of all Peary's observations on the route to and near the Pole, with results in harmony with the others, but gives neither results nor reference to any publication; and finally that a still more careful study was made by the American experts Mitchell and Duvall, which is published in Appendix C, and was the authority for the result on p. 357.

Let us then turn back to the narrative of what Peary did. On arrival he made the first sun observation at "noon"; after some hours of sleep he turned out for a "six o'clock evening observation, Columbia meridian time, but found the sky overcast," so he "went on" an estimated distance of 10 miles, took a "midnight observation" from the new position and found he had gone beyond the Pole. He returned to Camp Jesup, and at six o'clock on the morning of the 7th took a third series of observations in a direction at right angles to those already taken there. These, combined with the others, showed that the camp was 4 or 5 miles from the Pole in the direction of Bering Strait. Finally, after a walk of 8 miles poleward and back, he made another series at Columbia meridian noon of the 7th.

All this, derived presumably from Peary's records, is quite clear and straightforward, and shows that he was following most carefully the usual routine of morning, noon, and evening sights, except for the large assumption that close to the Pole he was still on his Cape Columbia meridian; but that made no real difference: so long as he got sights in two directions more or less at right angles he was all right, and evidently realized it after his morning observation. The only trouble is that he does not seem to have taken the watch time of each sight. His four sets are timed April 6, 12.50 p.m.; April 6–7, midnight; April 7, 6.40 a.m.; and April 7, 12.40 p.m. according to Appendix C, where it is assumed (doubtless on good authority) that his watch was keeping 60th meridian time; it is further assumed (on no stated authority) that his watch was 10^m fast at that time. As the Cape Columbia meridian is 10° (40^m) west of the 60th meridian, and Peary refers to Cape Columbia noon, it looks as if he planned his groups of observations for 6 a.m., noon, midnight of that meridian and neglected, being most likely ignorant of, the watch error. The first sight was made in broken sky, a single observation of the lower limb.

We conclude that the times attributed to Peary are those at which he intended to take his sights: not those precisely at which he did take them. The uncertainty is of no importance, since local time close to the Pole has little meaning. Considered formally, it makes Mr. Mitchell's calculation on p. 471, by the method of two altitudes and the elapsed time, look rather ill-founded, until one remarks, with Chauvenet ('Spherical and Practical Astronomy,' vol. I, p. 257), that the elaborate trigonometrical calculation is only a complicated way of solving the position-line problem, which, as was observed in this *Journal* in March 1910 and July 1926 (35.299 and 68.58), has near the Pole a great simplicity. Instead therefore of following Mitchell, who works to two places of decimals of a second, we can do in half a page a graphical solution which is more than sufficient for the possible accuracy of the observations, and has the great advantage that it shows exactly what happened.

The observations, reduced to G.A.T. (reckoned in 1909 from Gr. Noon) are—

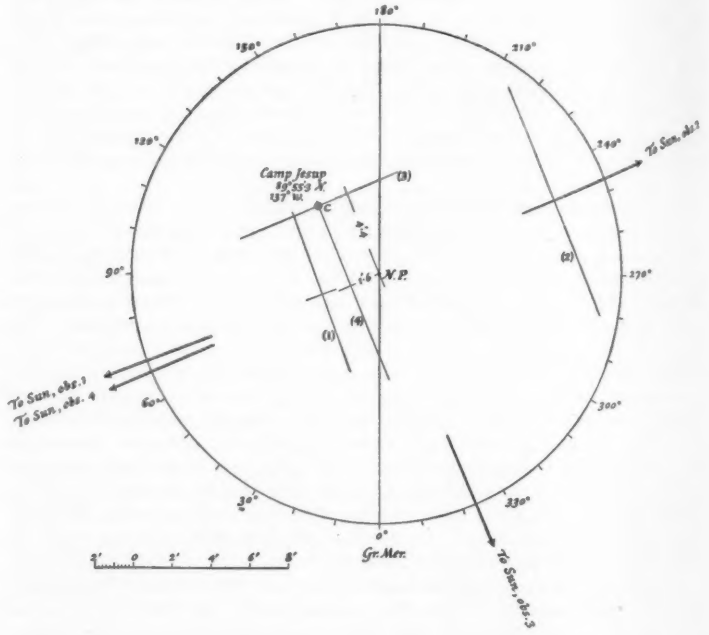
	1	2	3	4
G.A.T. Apr. 6 ..	$4^h\ 37^m{\cdot}6$	$16^h\ 27^m{\cdot}6$	$22^h\ 27^m{\cdot}4$	Apr. 7 $4^h\ 27^m{\cdot}6$
	$=69°{\cdot}4$	$=246°{\cdot}9$	$=336°{\cdot}9$	$=66°{\cdot}9$
Decl. of Sun ..	$+6°\ 23'{\cdot}3$	$6°\ 34'{\cdot}5$	$6°\ 40'{\cdot}2$	$6°\ 45'{\cdot}8$
Corr. Alt. of Sun ..	$6\ 26'{\cdot}5$	$6\ 44'{\cdot}3$	$6\ 35'{\cdot}8$	$6\ 47'{\cdot}4$
To/from Sun ..	$3'{\cdot}2$ to	$9'{\cdot}8$ to	$4'{\cdot}4$ from	$1'{\cdot}6$ to

These give in the simplest possible way the four position lines.

No. 1 has less weight than the others, being only a single sight.

No. 2 was observed about 10 miles from the camp.

Nos. 3 and 4 are the principal lines. They intersect at C and give for the position of the camp: Lat. 89° 55′·3 North, Long. 137° West., which agree as nearly as is worth while with Mitchell's 89° 55′ 22″·24; 137°.



One may be allowed to say that Duvall's calculation, by a different method, determining the hour angle of the sun from its observed altitude, arrives at Mitchell's results only because it starts by adopting his latitude, within 0″·06; without that advantage it would have produced something very different.

It has seemed worth while to examine this question afresh because the graphical method sets the observations in their true light. The results are perfectly consistent with the narrative; the sight at Cape Columbia midnight in the eastern hemisphere was a happy touch; and one could not expect in the circumstances any much better set of observations to prove that a man had been within a few miles of the Pole, except that he should have recorded the watch

times for each sight. What is most remarkable is that Peary, having had observed, so far as we are told, only latitudes, or, rather, position lines about local noon athwart his track, on the march northwards, kept so very close to the Cape Columbia meridian; it was a fine piece of steering. Only one or two of these earlier observations have, so far as we know, been published. If ever the full original records are made available to students, it will be a pleasant exercise to reduce them by the above method, and see what light they throw upon the matter.

Enough has been said to show how much more can be written in support of Peary, and in his praise, than has been done in this biography even by such a whole-hearted admirer as Professor Hobbs. Influenced unconsciously, perhaps, by those who have agitated against Peary, he has tended to insist too little upon the figures which they attack. We see no reason why Peary's rate of travel after he parted from Bartlett should not be emphasized. Hobbs implies that his rate was not unusual by giving in an appendix the records of some other fast journeys made, at least largely, upon sea-ice. But these are not really comparable, as so many are either homeward rushes or on familiar trails along a coast. Peary's great achievement was to have travelled over hummocky and often active pack-ice far from land at a speed far beyond his predecessors. His picked Eskimos and dogs made it possible; his own will and ambition and the knowledge that he was winning carried him through to his great triumph.

A. R. H.
J. M. W.

THE VALLEY OF THE LOWER MEUSE

LA BASSE-MEUSE: Étude de morphologie fluviale. By M. A. Lefevre. Société belge d'études géographiques, Mémoire No. I. *Louvain: Institut géographique de l'Université*, 1935. 10×7 *inches;* 192 *pages; maps and diagrams*

THERE are few questions which have evoked more discussion and more diversity of opinion than that of the interpretation of river terraces. The very significance of the term is still not generally agreed upon, and Mlle. Lefevre is wise in prefacing her scholarly study of the forms of fluvial erosion and deposition in the valley of the Lower Meuse by a reasoned introduction in which she makes clear both her definitions and her point of view. The latter is best expressed by saying that the work is a purely morphological study, carried out for its own sake. The author maps land-forms as a geologist maps formations, and terraces are consequently, and rightly, regarded and studied as morphological, rather than lithological or stratigraphical units. In the past the study of river terraces has undoubtedly suffered through being pursued by workers in many different fields, with very divergent points of view, and using very different definitions. Consequently, no definition exists which is adequate to the purposes of geomorphologists, and Mlle. Lefevre therefore proposes to define a river terrace as *une ancienne plaine fluviale surélevée, en form de gradin, au-dessus du lit fluvial actuel*, a definition which the reviewer warmly commends. Thus defined, the development of a terrace evidently involves two clearly distinguishable phases—firstly, the formation of a continuous flood plain, and secondly, the incision of the talweg below it. Such a terrace thus marks a definite and critical stage in the valley history, namely the moment when the equilibrium conditions of maturity represented by the flood plain are interrupted by a sudden return to the conditions of youth; it is definitely cyclic in origin.

Not all terraces however possess cyclic significance, and Mlle. Lefevre distinguishes two groups of non-cyclic forms, which she calls respectively "false" and "local" terraces. By the former term she designates the forms that result when an aggraded surface is built up by a stream overloaded as the result of excessive gullying by tributary streams, glacial discharge, or the like process, and subsequently dissected. By the latter she implies those minor terrace features which arise here and there as a flood plain is lowered in normal fashion. They include the *surfaces polygeniques* of Chaput and presumably such terraces as those discussed by Challinor a few years ago.[1] These distinctions appear to be well founded. Both groups of forms lack the continuity of extent which characterizes the terrace of cyclic origin and both are relatively ephemeral. The "false terraces" retain their original characteristics of form in recognizable fashion throughout most of their existence; the "local terraces" are soon destroyed by the same processes which produced them, and represent merely chance moments in flood plain history.

Not only are cyclic and non-cyclic forms separated but a further and most important distinction is made among the cyclic forms themselves. Clearly, since such terraces are former flood-plains now abandoned and dissected, we must distinguish between those which are formed from flood-plains of fluvial erosion and those which arise from flood-plains of fluvial deposition. The flood-plain of a mature river is, as is well known, essentially a planed solid-rock surface which is steadily lowered throughout maturity by flood-scouring

[1] J. Challinor, "River terraces as normal features of valley development," *Geography*, vol. xvii, 1932, pp. 141–7.

of the stream bed and the continued migration of the channel over the whole flood-plain surface during the sweeping and swinging of the meanders. Its longitudinal and transverse gradients, as Mlle. Lefevre does well to emphasize, are subject to continual change during this process, until in old age the rate of change at last becomes so slow as to be negligible. The transverse profile then becomes virtually flat and the longitudinal profile reaches what may be regarded as a limiting gradient (*la pente limite*), almost rectilinear and almost flat. At all times the bed-rock surface is covered by a relatively thin sheet of alluvium, whose thickness is sensibly constant over the plain and rarely exceeds a few metres. The material of this gravel sheet is stated to diminish in coarseness from base to surface and from the valley sides to centre, but the reviewer's experience suggests rather the desirability of distinguishing between a lower and an upper deposit, each of which remains fairly constant in character. The lower, which may be termed the "flood-plain ballast," comprises the coarser materials of the traction load abandoned during low-water periods; the upper member, "the flood-plain silt," comprises the fine material derived from the suspension load and deposited in the slack water of the inundated plain in time of flood. Such a plain showing this characteristic assemblage of features Mlle. Lefevre terms *une plaine fluviale d'érosion*. We may translate this as "flood-plain of degradation."

In contrast to such degradational flood-plains are the plains of fluvial deposition, According to Mlle. Lefevre, whenever the gradient of a stream becomes less than the limiting value, the stream becomes incompetent to transport all its load, and deposition ensues until the profile is built up to the "limiting gradient." The resulting *aggradational flood-plain* is illustrated by a diagram in the text (Fig. (4)). This diagram seems open to criticism in certain respects, especially in regard to the mechanism of deposition implied, but it is clear that terraces originating by the dissection of a flood-plain of aggradation will differ fundamentally from those cut from a degradational flood-plain. The longitudinal gradient will in general be less, the transverse profile convex upward, the mechanical grading of the material strikingly different, and instead of the small but relatively constant thickness of silt and ballast resting on a planed rock-floor, we shall find rapid and large changes of thickness in the deposit as we move upstream or toward the former valley sides, and a rock-floor may not be, and commonly is not, exposed at all. These then are the diagnostic characters which Mlle. Lefevre applies in her examination of the Lower Meuse. On the whole they seem well founded, and the account given of river features in the first section of her work is written with admirable good sense, and is deserving of attention by all who are interested in any of the many problems with which the question of river terraces is associated.

The middle and largest section of the work is devoted to a critical analysis of the land-forms actually developed in the Meuse valley below Liège. They are grouped by the author into four "fluvial complexes," each of which comprises all those elements—flood-plains and associated slopes, flood-plain gravels, and residual deposits, etc.—which are shown by the field evidence to be contemporaneous. The first complex (Cycle IV) comprises a series of high-level flats at 180 metres or just above, sometimes carrying old leached and patinated alluvia, and bordered by the rising ground of Condroz, Herve, and the Eifel on the south, and of Dutch Limburg on the north. On the south the flats are in part margined by a well-marked feature known to German geologists as the "Gebirgsrand," and in the past variously interpreted as a marine cliff, or fault- or fault-line scarp. Capping these bordering elevations over wide areas is a well-known gravel, *le gravier blanc*, which is regarded by the author as the

leached and impoverished remnant, comprising only the most indestructible elements, of a formerly widespread gravel formation which has been swept from all the valley lines and left only on the much-degraded interfluves. The surfaces on which these "graviers du facies residuel continental" rest are interpreted as parts of an old erosion surface which in Hesbaye and Dutch Limburg does not rise above 220 metres, *i.e.* not more than 40 metres above the old Meuse valley represented by the high-level flats. A convincing picture is thus given of a former senile landscape in which the old valley of the Meuse can be traced across country in an easterly direction from Liège to beyond Aachen at a level which is sensibly constant at about 180 metres throughout. At this stage the Meuse was evidently tributary to the Rhine, but at the next stage we find it pursuing its present independent course northward by Maastricht. The diversion is regarded as being prepared by the senile wanderings of the Meuse in the 180-metre plain, and actually effected by a small north-flowing tributary at the period when uplift initiated the next cycle. The author envisages a uniform drop of base-level at this stage, but the reviewer wonders whether some degree of northward tilting may not have been involved.

The forms of the later cycles bear a certain family resemblance to each other. Each comprises a degradational flood-plain upstream which is now dissected into terrace form, and in the lower course a widespread area of aggradation. It is on the latter that interest is chiefly focused. In the case of Cycle III this feature comprises an immense field of gravel occupying the Campine and much of south Holland, and stretching eastward towards the Rhine. It is regarded as having the form of an enormous flat cone with its apex a little north of Maastricht at about 130 metres. Corresponding to it are the aggraded floors of the tributary valleys cut below the old 180-metre surface. The forms of Cycle II comprise a terrace between Maaseyck and Liège, where it passes upstream into the undissected flood-plain of the present Meuse, and a great gravel spread below Maaseyck which includes the low plateau of the Peel. Finally, the modern flood-plain, Cycle I, similarly comprises two portions—the upper one a degradational flood-plain, presumably terminating upstream at the knick-point just above Liège, the lower one a plain of aggradation comprising the endiked lands below Grevenbicht, which are slightly incised below the level of the gravel plain of Cycle II. It is almost without gradient (0·09 per cent. at Venloo) and is composed of the finest stratified sand.

The field relationships of the forms and deposits of these three cycles and the preceding high-level cycle are well set out in a series of transverse sections (Plates I, II, III) and in a very convincing morphological map (Plate V). Their interpretation is attempted in the third major section of the book, and is prefaced by a fairly lengthy discussion of the divergent views of previous workers. Chief among these are A. Briquet, who recognized a series of fourteen "terraces" based on the levels of existing gravel patches; W. C. Klein, who found it necessary to invoke recent faulting in order to explain the present disposition of the Meuse gravel spreads; P. Fourmarier, who argued, from the altitudinal variations up and downstream of the terrace seen at Liège at 120 metres, that the present gradients of the terraces reflect deformation by an anticlinal axis passing through Liège; and L. Mouchamps, who followed Fourmarier and endeavoured to extend the scope of his hypothesis. All these are ably discussed and appear to be sufficiently refuted. There certainly is no reason to attribute any of the morphological features described to recent faulting, and the evidence for anticlinal uplift is both scanty and disputed. Mlle. Lefevre believes that all the observed features of the Meuse valley are capable of explanation in terms of normal cyclical development accompanied by changes of base-level. The

explanation offered is however not entirely convincing, and is least so when it deals with the aggradation phases.

The relations between the longitudinal profiles of the four stages recognized are set out schematically in a diagram and in more detail in Plate IV. Both of these reveal an unexpectedly steep gradient for the "alluvial cone" of Cycle II—some 2 °/₀₀ (two per thousand).[1] This gradient is regarded as original—partly because subsequent deformation is held to be precluded by the constancy of altitude of the relics of the previous cycle, and partly because the coarseness of the alluvial material is held to require a steep gradient for its transport. But this material must somehow have been carried down the more gently graded upstream reaches, and apart from the effects resulting from loss of volume by a stream breaking up into distributaries upon an aggrading plain, there seems no reason for supposing the gradient of the surface upon which the gravel was deposited should be steeper than that of the surface over which it had already travelled. The case of the abandoned alluvial cone of the Durance—la Crau—is cited to support the contention that the whole of the gradient of the former Meuse is original.[2] The gradient of la Crau is 3·2 °/₀₀, that of the Meuse "alluvial cone" 2 °/₀₀. But the gradient of the modern Durance above the point where the cone must have headed is still 2·65 °/₀₀, and if Baulig's contentions are just, the gradient at the time when la Crau was built was almost continuous from Sisteron to the sea.[3] But measuring from Mlle. Lefevre's diagram (Plate IV), the gradient of the terrace III of the Meuse from Maastricht upstream appears to be only 0·15 °/₀₀, a value less than one-tenth of that found below Maastricht. The gradients are obviously not of the same order and the differences are surely too large to permit of their having developed simultaneously in successive reaches of one and the same stream.

An attempt is made later in the book to explain this disparity, and to help this explanation a diagram is introduced whose essential features may be described as follows. Imagine a mature profile of fluvial erosion—the profile developed after the negative movements of base-level which separated Cycles IV and III. The lower part of this imaginary profile is then modified by aggradation, the degradational flood-plain being buried by the development of an alluvial cone. Degradation however still continues upstream, so that in essence the upper portion of the profile is being reduced in gradient more rapidly than the aggrading lower reaches. The diagram shows successive stages in this process by a series of profiles. Actually these are incorrectly drawn, inasmuch as the cones labelled I, II, and III all abut against the line of the original profile, and not against the lines representing the profiles with which they are supposed to be contemporaneous. Nevertheless, even if this error be corrected, and we accept the mechanism of aggradation there shown, it still seems impossible to concede that the rising and receding head of a growing alluvial cone can act as a base-level in relation to which the upper reaches of a considerable river like the Meuse can be reduced to a degree of slope lower than that of the cone itself, and still less to the very low value actually involved.

This however is not the whole difficulty. The gravels of the alluvial cone pass far below sea-level in Holland, and since they are practically without

[1] Actually reference is made to "la pente de la surface de base et de la surface terminale du cailloutis, respectivement 1·5 °/₀₀ et 2 °/₀₀" (p. 143). These gradients are difficult to reconcile with the downstream thickening of the deposits which is noted on p. 72.

[2] Page 134.

[3] H. Baulig, "La Crau et la glaciation wurmienne." *Annales de Géographie*, 1927, pp. 499–508.

admixture of marine debris and were presumably accumulated under sub-aerial conditions, it becomes necessary on this hypothesis to assume that sea-level throughout the whole of the aggradation period was much below its present level, being far below it when aggradation first replaced downcutting and still below it when renewed negative movements initiated the succeeding cycle. We are thus offered the astonishing conclusion that the well-marked terrace of the Meuse, which near Liège occurs at about 120 metres, was developed in relation to a sea-level considerably lower than that of the present day. To accept such a contention is surely to abandon the very beliefs which render the elucidation of denudation chronology possible.

This course however does not seem to be the only one open to us, and the reviewer suggests that many and perhaps most of the difficulties involved may be met by assuming that we are dealing not only with successive movements of base-level, but also with local tilting, which has resulted in cumulative depression of the northern areas (Holland) relative to the more stable palaeozoic massif of the south. Such tilting would indeed be no new thing. It has continued intermittently in this region and in this sense since cretaceous times at least, as is well shown by the southward convergence of the cretaceous and tertiary time-planes in a north-south section through Liège published by Fourmarier.[1] It would explain at once the depression of the older river gravels far below sea-level, their burial by the gravels of the later cycles, and the break of gradient which is otherwise so difficult to understand. The arguments against tilting, on the other hand, seem to have little weight. The coarse grade of the gravels does not necessarily imply so steep a slope as is actually found, and the argument drawn from the constancy of the level of the relics of Cycle IV is of course only valid within the region where these are found. They are, in point of fact, strictly confined to the area south of the latitude of Maastricht, and there is no reason to suppose that a result drawn from the southern area can safely be applied to the northern region of contrasted constitution and tectonics.

One further point arises from these considerations. From her interpretations of the land-forms Mlle. Lefevre draws the general conclusion that each of the Cycles III, II, and I comprised an oscillation of base-level, positive movement succeeding negative in each case. These oscillations may well be valid, as may also be their correlation with stratigraphical chronology of the Pleistocene. But the contention that effective base-level has throughout Pleistocene times been below modern sea-level is clearly not justified by the evidence brought forward to support it.

The work is rounded off by a chapter on the erosion features and alluvial surfaces of the Lower Rhine valley, amply demonstrating the presence of a series of "complexes" corresponding to those of the Meuse and presumably resulting from an analogous history. This is followed by a brief chapter which discusses the main conclusions that have emerged in relation to the wider problems of Pleistocene chronology and palaeogeography. Those conclusions are however not the most important thing about the book. Much more valuable is the brilliant piece of descriptive morphology which provides the material upon which the conclusions are based. Sound observation and sound induction are here allied to lucid presentation to provide an account which automatically takes rank as the basis for future morphological discussion of the region. But the book has an interest and a value which demand that it should be read not merely by workers in this particular field, but by all those in this country who

[1] P. Fourmarier, "Vue d'ensemble sur la géologie de la Belgique," *Annales de la Société Géologique de Belgique*, Mémoires in 4°, année 1933–34, Fig. 49, p. 177.

value honest observation and clear thinking about morphological problems. It has no like in our language: there is no British river for which such a body of observation and induction has been gathered together. The desirability of such purely morphological accounts has in fact been inadequately recognized in this country, and it is to be hoped therefore that many will read this book and some at least will be inspired to emulate it. DAVID L. LINTON.

THE LAST OF THE ZERZURA LEGEND

RÉCENTES EXPLORATIONS DANS LE DÉSERT LIBYQUE (1932–1936). By L. E. DE ALMÁSY. (Publications de la Société Royale de Géographie d'Égypte.) *Le Caire: E. and R. Schindler (for S.R.G. Égypte)*, 1936. $9\frac{1}{2} \times 6\frac{1}{2}$ *inches; 98 pages; illustrations and route-map.* [*London: Quaritch.* 7*s*]

THE name Zerzura has appeared so often in the pages of the *Geographical Journal* in connection with a legendary oasis somewhere unlocated in the heart of the Libyan Desert that the *Journal* would be incomplete were the latest information to be omitted. This consists of an interesting statement obtained from an old Arab of the Zwaya tribe, native of Kufara, by Count Almásy and included in his book, which the Société Royal de Géographie d'Égypte has recently published, entitled 'Récentes explorations dans le désert Libyque.'

Briefly stated, the Zerzura problem was as follows: The first reference to the name by a European writer occurs, as far as is known, in a book by Sir Gardiner Wilkinson published in 1835. Here it appears as the name of one of several oases said to exist far away in the west beyond the farthest known Egyptian oasis. Wilkinson connected it with a story of an "Oasis of the Blacks," whence a former invasion of the Western Oases by black men had taken place. He stated that it was discovered by an Arab about nine years previously to 1835. His account was made the more convincing by the subsequent discovery, in the Kufara group, of the other oases he mentioned by name; and the invasion by the Tibu of the central desert is confirmed by the memories of the people of Dakhla Oasis. The source of Wilkinson's information is not known, but researches have since established that the name Zerzura occurs in Arab writings as far back as the thirteenth century.

As exploration of the farther desert revealed other oases such as 'Uweinat, of which no living Arab on the Egyptian side appeared to be aware, interest in Zerzura increased as the field was narrowed. But as the search extended into the present century the sources of information dried up, and such reports as were collected from the Badawin dwindled to vague stories of something seen in the distance by wandering men looking for lost camels.

It must be remembered that until only a few years ago Egypt as known to Badawin and European alike faded away beyond the western confines of the Oases into a limitless horizon of sand-dunes. Although Senussi caravans from Kufara had from time to time arrived out of the west, the people of the Egyptian side seem to have taken no part in these journeys. By Egyptian standards there was no water west of the Oases, and the world ended. The black Tibu, who occasionally appeared out of nowhere, speaking an unknown tongue, were regarded as not quite of this world. It must also be remembered that the Arabs,

for all their fame as camel-masters and desert travellers, never really penetrated into the Libyan Desert for twelve centuries after they had overrun the whole North African coast. It was only within the memory of living men that Kufara was occupied by Zwaya Arabs from Cirenaica. To the Arab tribes the interior was an utterly waterless desert without grazing, in which roamed dark beings who either lived without water or possessed wells the whereabouts of which they guarded with the greatest secrecy. It was from the higher standards of desert travel of the mysterious Tibu, with whom the Zwaya first came into contact when they arrived at Kufara, that the Senussi must have learned to make those prodigious caravan journeys of 360 miles across the dunes into Egypt.

In 1923 the late Prince Kemal-el-Din, on his first journey to 'Uweinat, discovered the great rock plateau of the Gilf Kebir, passing along the foot of the eastern cliffs for more than 100 miles. But it was not until eight years later that P. A. Clayton, working northwards from 'Uweinat, followed the western cliffs of the plateau and traced its general form. Later that same year a party led by Almásy, and consisting besides of the late Sir Robert Clayton-East-Clayton, Wing-Commander Penderel, and P. A. Clayton, set out to explore the Gilf with cars and a light aeroplane. During a flight over the plateau two of the party sighted a wadi containing live green trees; but subsequently from the desert beneath the cliffs they were unable to find the wadi's mouth. Upon hearing of this discovery from Almásy on his return to Cairo, Prince Kemal-el-Din mentioned that he had heard tell of this wadi, and that its name was Wadi Abd-el-Melik. This was very shortly before his death, and he apparently never amplified his statement.

Since then the wadi has been visited by P. A. Clayton, who also found a second wadi to the east of it. In 1933 Almásy found a third wadi to the west. On a visit to Kufara, Almásy met a Tibu who knew all three wadis (the skeleton of his camel lay in one of them), and said that they were called Wadi Abd-el-Melik, Wadi Hamra, and Wadi Talh; after rain had fallen—every seven years or so—there was excellent grazing there, to which they drove their camels and those of their Zwaya masters.

In 1935 Almásy learned that among the refugees who left Kufara on the Italian occupation in 1931 was an old man named Abd-el-Melik, who was now pasturing camels in the Faiyum. After a year's delay this man was at last located.

"Abd el Melik," says Almásy, "may be about 70 or 75 years of age, he is of the Zwaya tribe, and his home was the oasis Zurk at Ḳufara. He was a poor man all his life and has never owned land or date palms in Kufara, but had always wandered about pasturing the camels of wealthier people or acting as a guide to caravans.

"He had travelled along those hardest and most dangerous desert routes, Kufara-Abu Mungar and Kufara-Siwa, in both senses about a dozen times, he had accompanied the Senussi to Goru in the Waddai and visited the northern Dar-fur, the Tibesti and 'Uweinat. Six months before the Italian occupation of Kufara he had come to Siwa and had been living in the Egyptian oases and in the Faiyum ever since. His health seemed to be perfect and he astonished me by his ability—quite exceptional with Arabs of his kind—of recognizing photographs of people and places that he knew. In the course of our discussions he even drew a map with pencil on a large sheet of paper, which was astonishingly accurate, both in scale and in orientation."

There follows a word-for-word rendering of the old man's story of his visit to the Gilf wadis, prefaced by an assurance by Almásy that "I anxiously avoided influencing the old man, and no place-name was mentioned and no question

asked until his own story was put on paper." The story is sufficiently interesting for a portion of it to be quoted.

"I Abd el Melik Ibrahim el Zwaya have the following to say concerning the valleys which I discovered:

Kufara Oasis did not always belong to the Arabs. From long ago it was the land of the Tibu, who owned all the places in the desert for ages. When the Arabs and the Senussi came to Kufara, the Tibu left the oasis and went to the Tibesti mountains that lie fifteen days' camel march to the south. Later, when the Senussi began to build houses in Kufara, the Tibu returned one by one and lived among the Arabs, but they still owned other places which the Arabs did not know. It was the Tibu who informed the Arabs that there were grazing grounds in 'Uweinat and also pastures and palm trees in Merga. They also knew other valleys with good grazing such as Zerzura, but no Arab knew that place. We only heard that there was a valley with an oasis called Zerzura situated to the south of Siwa, east of Kufara, north of 'Uweinat and west of Dakhla. We also heard that some people who lost their way in the desert had once found Zerzura and that they saw palm trees and water springs there.

The Senussi sent out people to search for all these places, and a certain Mohamed Lefeitar was sent to search for Zerzura, but he returned to Kufara without having found the place."

He goes on to tell how one day many years ago, when camels were in demand at Kufara, a certain Tibu, disgruntled with his tribe, gave away to the Senussi the secret whereabouts of the Tibu camels. Abd-el-Melik was thereupon sent out with a companion to discover the place and seize the camels. There follows an account of his four-days' journey to the south-east and a most accurate description of the Gilf Kebir and of the Wadi Abd-el-Melik as seen by Almásy, Clayton, and others. In only one respect is there a difference. Abd-el-Melik describes a spring, of which he says, "The spring lies near the upper end of the wadi, on the left-hand side when going up, not very far after one has passed the heglig trees on the right-hand side. The water is good and abundant and I believe there is always water there like in the springs of 'Uweinat."

Finally he says, "There are mountain sheep and foxes and many small birds in the valleys, and I believe that it was for this latter reason that the valley was called Wadi Zerzura before."

There can now be little doubt that the wadis in the Gilf Kebir are the truth behind the Egyptian legends of the "Oasis of the Blacks." The actual water-hole has not yet been found, but it most probably will be when next the wadi is visited. Almásy deserves very great credit for his persistence in following up the problem of Wilkinson's oasis and for the success of his efforts.

The origin of the name Zerzura is however still obscure. How comes it that the name occurs in Arab writings so many centuries before the place was discovered by an Arab? And how is it that at the date given by Wilkinson for the discovery by an Arab, as also at the date of Wilkinson's book, Abd-el-Melik could not have been born? There is here a serious discrepancy.

I shall continue to think that Zerzura is one of the many names that have been given to the many fabulous cities which the mystery of the great North African desert has for ages created in the minds of those to whom it was hardly accessible; and that to identify Zerzura with any one discovery is but to particularize the general. Surely the most likely reconstruction is this. Wilkinson's Arab did in truth discover the Gilf wadis, and, Zerzura having been for centuries the local generic name for undiscovered places, he naturally called it Zerzura. That was in 1826 or thereabouts. In 1884 the catastrophe in the Sudan closed

the Arba'in slave road, and desert travel ceased over the whole area between the Nile and 'Uweinat. Thus if any of the Badawin concerned in the former Arba'in traffic still knew of the Gilf Kebir and of 'Uweinat itself in 1884, the memory of them would have died out by the beginning of this century, leaving the desert empty again for Abd-el-Melik to make his separate discovery.

Assuming that his word Zerzura came from the old man's memory of ancient tales, and did not reach him lately through the hearsay accounts told him by his friends of Almásy's efforts to find him during the year before they actually met, Abd-el-Melik must in his turn have associated his wadi with Zerzura for just the same reason as did Wilkinson's Arab.

'Récentes explorations dans le désert Libyque' is limited to an account of the author's travels. Much of it is taken up with the Zerzura legend and with some interesting but somewhat speculative interpretations of statements by Herodotus concerning the lost army of Cambyses and other matters. More important are the author's discoveries of rock paintings both at 'Uweinat and in the Gilf Kebir. The book is illustrated throughout by remarkable reproductions in colour of these ancient paintings. R. A BAGNOLD.

REVIEWS

EUROPE

UNDER JANE'S WINGS. By ELLEN BARBARA FLOWER. *London: Edward Arnold & Co.*, 1936. 9×5½ *inches;* 314 *pages; illustrations and route-maps.* 16*s*

"Jane, thirty tons, with a length of fifty feet, a beam of fourteen feet, and drawing eight feet six, we believe to be the largest boat without a motor, that a man and a woman ever took out of British waters." This is the opening paragraph of an exciting narrative of the cruise of a Bristol Channel pilot cutter from Plymouth to Marseilles, and the experiences of the owner and his wife are well worth recording. The skill of the navigator, his courage and powers of endurance, are remarkable, and throughout the voyage of four years his cheerful wife nobly supported him, although at times she was overcome by sea-sickness. Mrs. Flower, the narrator of this tale, has the sensitive vision of an artist and unusual powers of description which have enabled her to produce a book which is much more than mere diary of an eventful voyage. In a few words she can depict a scene for example: "On towards Clovelly with its thin stream of houses cascading eternally down into the sea"; and again: "In the evening we passed the mountain of Zarafaya, the foothills are all dimpled and delled like so many honeycombs, and in the evening light each hollow became a pool of blue." These Elizabethan adventurers may be described as "bylanders," an old term for coast-wise cruisers. They followed the coast-line except when bad weather made them put out to sea in order to safely ride out a gale. They were well supplied with charts and sailing directions, and navigated by sea-marks and land-marks; they had had no lessons on navigation, and their first introduction to the sextant was the gift of one by a naval officer at Gibraltar. Nevertheless the sea-sense of the skipper carried them through, and they weathered the storms of the Bay and the Gulf of Lyons without any grave disaster. The voyage was a leisurely one; the first winter was spent at Morlaix and the second at Arcachon, and in both these anchorages they had repeated difficulties owing to their great draught. The third they spent at Gibraltar, where they enjoyed the social life of the Rock; here the skipper filled up his spare time by painting portraits. They seem to have had a charming way of endearing themselves to the inhabitants among all sorts and conditions of men, and received willing help from the French and Spanish fishermen, and occasionally they shipped one of them to pilot them into some of the difficult harbours into which no yacht had previously ventured. The description of both people and places and the historical associations connected with them makes very interesting reading, and we heartily recommend this book to any one interested in foreign travel, and especially to fellow yachtsmen.

H. M. E.

THE PEACE SETTLEMENT IN THE GERMAN POLISH BORDERLANDS: A study of conditions to-day in the pre-war Prussian provinces of East and West Prussia. By IAN F. D. MORROW, assisted by L. M. SIEVEKING. Issued under the auspices of the Royal Institute of International Affairs. *London: Oxford University Press*, 1936. 8½×5½ *inches;* xiv+558 *pages; map.* 25*s*

The object of this book is to present an account of the working of the Peace Settlement in the German-Polish borderlands. It was undertaken in 1932, and is based chiefly on visits to the territory concerned in 1932 and 1933, and on material printed for the most part before that date. A chapter on the Third Reich forms an epilogue, carrying the book down to 1935. The problem is

treated as primarily economic and political, and little attempt is made to deal with it in its geographical aspects. A mass of documents and statistics has been ably marshalled by the author; his work is readable as well as informative; and the account is likely to remove several popular misconceptions.

The opening chapter on the Eastern Marches usefully emphasizes the historical basis of Poland's claim to the Corridor, and explains the historical confusion which the Peace Conference was called on to clear up, taking its stand upon the Thirteenth Point which guaranteed Poland access to the sea. In his historical summary the author might have dealt more fully with the Partitions of Poland in the eighteenth century, of which the dates are not given.

Four chapters are devoted to a study of Danzig: a German island in a Polish sea, and ill-fitted to become even a Free City because it has always been conspicuously lacking in local patriotism. It is clear that the Germanity of Danzig was irreconcilable with democratic self-determination, and the rights of millions of Poles were accounted by the Conference of greater import than those of thousands of Germans. The result has been a series of disputes between Poland and Danzig, chiefly (but not always, as on the occasion of the Munitions Strike in 1920) over trivial matters and protracted by the obstinacy of both parties. Poland has created Gdynia to serve her, but that hardly seems to provide a solution since the Peace Treaties gave Danzig prior right to Poland's trade. The author hopes for a cessation of racial intolerance. By virtue of the Thirteenth Point, he regards Poland as an essentially maritime state and considers her need for a port paramount (p. 55): which seems to beg the question.

Chapters VI–XII deal with the yet thornier problem of the Corridor. Historically, economically, and ethnographically the Corridor is justifiable and for the Poles no problem exists. Moreover German national sentiment was not outraged by the actual loss of Pomorze (p. 184), although the problem of the Corridor (since transit is satisfactory) remains essentially emotional (p. 185): a subtle distinction. In short, it is East Prussia and not Pomorze which is the problem. It may be said at once that the losses incurred by East Prussia through its separation from the Reich do not appear to be extensive, and to be mostly due to the loss of trade with Russia and to the loss of the former Polish provinces (and thence irremediable). But even if it is easy to show that East Prussia was always a poor province, that agriculture never flourished, that emigration has always been extensive, it is undeniable that it symbolizes the German mission in the East, and is closely associated with the Prussian tradition. Its Junkers have always been able to influence German policy, and in the latter days of the Republic secured increasing attention for their woes. Mr. Morrow goes at length into the question of agricultural conditions in East Prussia, and devotes an entire chapter to agrarian indebtedness alone. As a whole he is inclined to favour the Junkers. To dispel the myth of the great estates, he draws a distinction between great estates (over 1000 hectares) and large estates (over 100 hectares). Of the former there are admittedly few; but as 39 per cent. (p. 257) of East Prussia is in the hands of the latter, it is a little misleading, by comparison with Silesia, to term East Prussia "a land of relatively small estates" (p. 259), especially in view of the statement on p. 316 that "East Germany is the home of large estates." The author justifies the existence of large estates by reference to Soviet farming, but the large estates in East Prussia are the most heavily indebted (p. 319). It is admitted that the small farmers there were on the whole the more successful. The author gives a variety of reasons—climatic, financial, and agricultural—for the increase of debt; compliments the Junkers on their "hard-fistedness and frugality" (p. 327); and finally admits that they were improvident farmers (p. 416), willing to make use of the *Osthilfe*. The section dealing with East

Prussian agrarian conditions is rather long. Summaries given on pp. 252–3 are repeated at length on pp. 265–8; but for pp. 268–90 no summary is given until pp. 350–1. Various mentions are made of the stoppage of river traffic and the breaking of communications, a subject which might be more fully dealt with. There is (p. 406) an eulogy of President von Hindenburg, which is to a great extent belied by the author's description (p. 413) of his policy with regard to the East and of his accessibility to Junker influence.

The chapter on Memel shows how the interests of an essentially German city have again been sacrificed, on this occasion to those of Lithuania, the problem thus created being fundamentally racial.

Finally the author deals with the Third Reich, and with the German-Polish Accord. He stresses the fact that office and responsibility have modified official views, but not every one will agree with that, in view of a recent reference to Lithuania which is quoted by the author.

There are three appendices, including one on the finances of Danzig by Miss L. M. Sieveking. The book is amply documented and indexed. The maps are disappointing, particularly the end map on Central and Eastern Europe after the Treaty of Versailles, which is crowded with unnecessary names. Mr. Morrow explains in his preface that it has been necessary to exclude chapters on conditions in the former German province of Posen and on the situation of German minorities in Poland. This seems a pity, as they would have rounded off the study.

Chatham House prides itself on impartiality. Mr. Morrow is not therefore concerned with passing judgments. Neither is it his business to provide solutions for the problems which he describes. He himself clearly looks for relief to the deadening of racial hatred, as the only alternative to forcible revision. Perhaps in historical questions the end must be considered to have justified the means, and the existence of historical wrongs should never be admitted.

J. P. H. M.

DU NOUVEAU SUR IDRĪSĪ. Sections VII 3, VII 4, VII 5: Europe septentrionale et circumbaltique, Europe orientale et, d'après quelques manuscrits, centrale jusqu'à la péninsule balkanique au Sud. Edited by O. J. TUULIO (TALLGREN). (Societas Orientalis Fennica. Studia Orientalia VI 3.) *Helsinki: Imprimerie de la Société de Littérature Finnoise* [*Leipzig: O. Harrassowitz*], 1936. $9^{1}/_{2} \times 6^{1}/_{2}$ *inches;* x+242 *pages. Key-maps and Facsimile MSS.* 180*Fmk.*

In this work Professor Tuulio reissues and expands two of his earlier Idrīsī studies, and he now covers all sections of the famous geographical treatise that relate to the countries of Northern Europe. The method he employs is still the same—to make a minute scrutiny of all available MSS. and maps, with an eye directed especially to likely points of calligraphic distortion, and on this basis to re-establish the original text as closely as possible.

The present reviewer cannot judge how far the identifications he puts forward are justified in the light of mediaeval European sources, but is competent to express an opinion only on the treatment of the Arabic text. And this opinion remains the same as that expressed in reviewing Professor Tuulio's first work on this subject. Fundamentally, his method and his presuppositions are sound. Idrīsī *did* write sense, his informants presumably *did* know what they were talking about, and much of the difficulty of interpretation *is* due to copyists' errors. But in reconstructing the text, a thoroughly good piece of work is marred by excess of criticism, and Professor Tuulio even has to admit in his notes (p. 143) that his reconstructions will not always hold.

The translation is painstaking and generally exact, but suffers from lack of flexibility. By overlooking the fact that *bilād* also (and commonly) means "towns," the latter half of pp. 16 and 17 is completely misinterpreted. "These lands" (says Idrīsī, of Finmark, Tavast, etc.) "are mostly open country and wilderness, with inhabited villages and perpetual snows, and their towns are few. As for Finmark, it is a land abounding in villages, cultivation, and flocks; but there are no populous towns in it except the city of Ābīr'ah and the city of Qalmār, and they are both big cities, but bear evident marks of rusticity," etc. This is all perfectly plain, and the laboured renderings given in the book, with their "emendations," do not inspire much confidence. Other passages also could be quoted, but this should suffice. After all, an Arabic text must be read as Arabic, and not treated as a miscellaneous collection of "palaeographic" symbols, to be twisted in any desired direction. H. A. R. G.

ASIA

PALESTINE ON THE EVE. By LADISLAS FARAGO. *London: Putnam* (1936). $8^{1}/_{2} \times 5^{1}/_{2}$ *inches;* x+286 *pages; illustrations and maps.* 10*s* 6*d*

The author of this book is a Hungarian journalist who spent some time in Palestine during the Arab riots which occurred in the summer of 1936. This book is an account of his stay, and attempts to give a rapid day-to-day account of events and problems in that remarkable land. It is not, and does not claim to be, a thoroughgoing analysis of the situation and its historical antecedents; it is a sequence of brightly coloured impressionist sketches of the recent history of the country, of the activity of and relations between its inhabitants, of the clash of character, ideals, and methods between the three dominant groups—the Arabs, the Jews, and the British Administration. The author, who had opportunities of meeting all sides to this dispute, used them well. In the very short time at his disposal he seems to have collected a mass of relevant gossip and information, which he has turned into a swift and racy narrative. The account of the facts is not always conspicuously accurate, the political structure of both Arabs and Jews is inadequately described, the character sketches are sometimes founded on faulty biographical information, now and then names are misspelt, but the style is lively and entertaining, and the book is extremely readable. The author is careful, after having stated the conflicting claims of Arabs and Jews as expounded to him by the rival spokesmen, to come to no conclusions of his own. His comments are, on the whole, singularly free from bias, at the same time their impartiality makes them neither colourless nor distant, and to those therefore who wish to read a short, entertaining, undistorted account of the more conspicuous facts, this book may confidently be recommended.

ARCHAEOLOGICAL HISTORY OF IRAN. By ERNST E. HERZFELD. (The Schweich Lectures on Biblical Archaeology, 1934.) *London: Oxford University Press (for the British Academy)*, 1935. 10×6 *inches;* xii, 112+xvii *pages; illustrations and map.* 7*s* 6*d*

It appears from his preface that Dr. Herzfeld aims at building up Persian history on archaeological evidence, a process which he regards as "an extension of the notion of archaeology" in some fashion which is not clear to the reviewer. The lay-reader of his book has difficulty in maintaining hold of the trend of history through a considerable mass of archaeological detail.

There are three divisions of the matter of the book in three lectures: Pre-Achaemenian and Achaemenian Epochs (to *c.* 330 B.C.); The Hellenistic Period (to A.D. 224); the Sasanian Epoch. Apart from some topographic and other

identifications, the most interesting thing in the first lecture is the discussion of the establishment in Iran of Mazdian religion. "Darius, Xerxes, and Artaxerxes were Zarathustrians, and the only true ones." Up to their time burial, and elaborate tombs for the great, were usual. After them exposure of bodies, as by the Parsees to-day, became general—a Magian custom, previously reserved to the priests; and the change coincided with attempts to fix the (Zend-)Avesta by writing. The Sasanian period is viewed as somewhat negative, a puritanical period (artistically) marked by reaction against Greek influence, preparatory to the more violent Muhammadan movement in the same direction.

A better map would have made perusal of these lectures much easier: the map provided is useless because it is reproduced inappropriately; as a line block it would have been legible. The photographs, of which there are some two dozen, are, on the other hand, very good. Unfamiliar spelling of quite common proper names is an irritation to the reader. A. S.

THE ASCENT OF MOUNT STALIN. By MICHAEL ROMM. Translated by ALEC BROWN. *London: Lawrence and Wishart*, 1936. 9×5½ *inches;* xii+270 *pages; illustrations and map.* 7*s* 6*d*

The Russian scientific expedition of 1933 to the Western Pamirs seems to have comprised no less than forty groups engaged on various investigations. Two of these were primarily mountaineering projects, set the official task of climbing "Mount Stalin," and of establishing an automatic radio-meteorological station at its summit, 24,590 feet. Of the two attempts, that from the east under Nikolai Gorbounov was successful, though the instruments were not taken to the top. Such an event as the ascent of the highest mountain in Russian territory, in a region so little known in this country, certainly merits a translation of the account into English.

The story is written by one who is evidently a novice in exploration and climbing, as appear to have been several other members and hangers-on. The author seems to have acted in the capacity of official observer from the base camp, and took no part in the assault. Judging from the reports of Rickmers' expedition, the present account much exaggerates the difficulties of the Fedchenko and its tributary glaciers, while everyday mountaineering methods are described in journalistic terms. There is a lack of appreciation of the work of great Russian explorers of Tsarist days. Definite statements, such as that on p. 109, that pre-war Russian scientists and explorers had not the necessary alpine experience or technique for Pamir exploration are contrary to fact. Nor is it true that the Pamirs were unmapped before the war, or that the Kara Kul lake was legendary. Russian geodetic triangulation crossed the Pamirs in 1912; it was broken down into secondary triangles, and topographical survey was in progress when the war broke out. Reconnaissance maps existed long before that. None of the great Russian explorers of the past would have attempted to cross swollen Pamir rivers in the late afternoon, or would have blamed the rivers for drowning in one season fourteen persons who elected to do so. Nor can one imagine Grombchevski, for instance, sitting up late at night, studying a textbook on mountaineering before tackling an obstacle, like a child cramming for an examination!

Insufficient credit is given to Rickmers' expedition of 1928, or to Finsterwalder, who mapped the whole region in detail. The map included is extremely crude and difficult to follow. This is all the more annoying owing to the ghastly system—or rather lack of system—in naming and renaming features already known from Finsterwalder's map. Darwas is renamed Pik Garmo, and Garmo is renamed Pik Stalin, the reasons given being far from convincing. Other

summits are plastered with the names of political characters of ephemeral notoriety; while Pik German-Communist Party is the inappropriate name given to a neighbouring Pik Ogpu. British geographers will be well advised to stick to Finsterwalder's names and heights for the time being.

The translator admits to a difficulty in transcribing native Pamir and Turki names into English. He says that it was open to him to investigate the local pronunciation and use some "scientific" method of transliteration; and he rejected that line. It was also open to him to adopt accepted spellings; he did not do so. The result is that there is no system at all and many of the places are not recognizable on any published map, while the text is disfigured by such monstrosities as Kouen-Louen, Hindou-Koush, Moustag-Ata, Kzyl-Art, all of which have definite and accepted spellings in English geography. There is a curious mistake on p. 221, where the date on the inscription cut on a rock at the base camp is given as 3 September, 1934. The expedition took place in 1933. K. M.

THE ENGLISH FACTORIES IN INDIA. Vol. I (New Series). (The Western Presidency), 1670–1677. By Sir CHARLES FAWCETT. Published under the patronage of His Majesty's Secretary of State for India in Council. *Oxford: Clarendon Press*, 1936. 9×6 *inches;* xxviii+390 *pages; illustrations and map.* 18*s*

This volume inaugurates a new series of the 'English Factories in India,' in which the contents of the India Office records are presented in less detail than in the thirteen volumes covering the years 1618–69 previously edited by Sir William Foster. Instead of dealing with the factories in all parts of India in the same volume, moreover, it has been decided that the records referring to (1) the Western Presidency (Surat, Bombay, Malabar Coast and Persia), (2) the Coromandel Coast (Madras, Masulipatam, etc.), and (3) Bengal will be treated separately.

This first volume deals with the settlements under the Surat Presidency (Bombay did not become the headquarters till 1687) during the years 1670–1677, a period of special interest, as it covers the administration of Gerald Aungier, the "true founder" of Bombay, and a very critical time in the history of the island. The Portuguese in their neighbouring settlements were definitely hostile, obstructing the trade with the mainland, while grave friction had resulted from the attachment by Sir Gervase Lucas in 1666–7 of the lands on the island held by Portuguese Jesuits and others. The activities of Sivaji in the Deccan in his conflict with the Mughal Government and the pillaging by his troops were a constant source of alarm and loss; Surat itself was sacked for the second time by Sivaji in 1670, the English factory only escaping destruction through the bravery of Streynsham Master helped by seamen from the ships. The annual visits paid to the harbour by the Sidi of Janjira, employed by Aurangzeb in his war with Sivaji, was another cause of alarm and quarrel. The war with the Dutch (1672–74) involved loss of ships, and necessitated measures of defence, while the Malabar pirates were a continual menace to the Company's vessels and coast factories. The island had thus but little rest from military preparations. Besides these external dangers, Aungier had to face disaffection and overt mutiny on the part of the troops, the misconduct and trial of some of the senior servants of the Company, and the then insalubrity of the island, which resulted in an appalling death rate among the Europeans, and so undermined his own health during the last four years that he died before the Company's assent to his retirement was received. These conditions, combined with an undeserved censure from home, were enough to dispirit any man less stout-

hearted and capable than Aungier. In spite of such embarrassments however he carried through many of the projects he had formed for the development of the settlement and the improvement of its administration. A few of these deserve mention. He achieved the famous "Convention" by which the dispute about the lands held by Portuguese was settled in perpetuity; he established the first Court of Judicature on British judicial principles in India; he set up the first British mint in India, where at first tin and copper coins were struck, and later silver and gold; and by raising a mobile force of fifty horse for the better guarding of the island, he anticipated the formation of "Light Horse" regiments that played so important a role in more recent times. He appointed to be Surveyor-General (the first, we think, to hold this title) an able German engineer, Col. H. Bake, who, according to the records, prepared "a large map of the island." Whether any copy of this map is extant we are not told.

This volume has been admirably edited, with a full index and adequate explanations of all vernacular terms, by Sir Charles Fawcett, whose personal acquaintance with the Presidency and its history has obviously made the task a congenial one. Had a map of the island and its immediate vicinity been added, it would have helped the reader to follow the narrative in places. C. E. A. W. O.

THE SNELLIUS-EXPEDITION IN THE EASTERN PART OF THE Netherlands East-Indies, 1929–1930, under leadership of P. M. VAN RIEL. Vol. II. Oceanographic results. Part 2. Soundings and bathymetric charts. Chapter I. Depth determinations. By F. PINKE. 68 *pages; diagrams* (1935). Chapter II. The bottom configuration in relation to the flow of the bottom water. By P. M. VAN RIEL. 63+18 *pages; charts* (1934). *Utrecht: Kemink en Soon*, 1934–5. $12^1/_2 \times 9$ *inches*

THE SNELLIUS-EXPEDITION IN THE EASTERN PART OF THE Netherlands East-Indies, 1929–1930, under leadership of P. M. VAN RIEL. Vol. V. Geological results. Part 1. Geological interpretation of the bathymetrical results. By PH. H. KUENEN. *Utrecht: Kemink en Soon*, 1935. $12^1/_2 \times 9$ *inches;* 124 *pages; maps and diagrams*

The publication of the geological and oceanographic results of the Snellius Expedition to the eastern part of the Netherlands East Indies, 1929–1930, is an event of first-class importance to all geologists and geomorphologists. In one of the three parts now published Commander F. Pinke gives a detailed technical account of the methods employed in sounding. Both wire soundings and echo soundings were taken, but the majority of the records were obtained by the latter method. The estimation of errors is considered in detail, and such errors are shown still to be considerable. It is important to notice that the slope of the sea-floor affects echo-sounding and that the same set of soundings can be interpreted in more than one way. P. M. Van Riel, the leader of the expedition, has set himself the task of interpreting the 30,000 new soundings and the 3000 old. A magnificent series of coloured bathymetric charts is the result—detailed ones of critical areas and two of special interest to the geographer. One of these shows the whole of the East Indies, the other (in northern and southern sections) the eastern part in greater detail. Much of the work on these was carried out by the Dutch Navy.

With all this material before him, Dr. H. Kuenen, the geologist of the expedition, is able to present a lucid account of the structure of the East Indies. He patiently analyses the hypothetical reconstructions put forward by Brouwer, Van Es, Molengraaff, Lawson, Lake, Staub, Wegener, and others, but curiously enough does not mention the very positive assertions of Professor J. W. Gregory on the structure of the Banda Arc (*Geogr. J.* 62 (1923)). It would seem to be

clear from the new evidence that Gregory's position is quite untenable. Surprisingly large sections of the enclosed seas are "deeps" of more than 4000 metres. Of these the Sulu Basin, the Celebes Basin, the North and South Banda Basins belong to what Kuenen calls his first type and which he notes have flattish floors, and believes to be down-faulted blocks or graben. The second type comprises elongated and more irregular basins and may be geosynclinal troughs. Opinions differ as to the age of these major features. Kuenen believes with Molengraaff that they are comparatively recent (*i.e.* Tertiary). Rapid changes in depth seem to indicate great fault scarps, and one of the most interesting and far-reaching conclusions made by Dr. Kuenen is that the present slopes on the sea-floor are such as would cause great masses of sediment to slide downhill and thus produce the features of intense folding, overfolding, horizontal faulting, and the formation of "nappes," now so well known from the structure of the Alps. Clearly the acceptance of this "sliding hypothesis" would revolutionize current ideas of "mountain building." L. D. S.

THE MARCH OF JAPAN. By EDGAR LAJTHA. *London: Robert Hale & Co.*, 1936. 9×5½ *inches;* 288 [*including* viii] *pages; illustrations.* 12*s* 6*d*

Literary fashions in regard to Japan have certainly undergone some striking changes. Thirty years ago writers concerning themselves with that country almost invariably dwelt on her more sentimental aspects, such as her flowers, children, festivals, moon-viewing; then they became more realistic, and more recently still they have tended to become Japan's hostile critics. If however Mr. Lajtha's work bears an intimidating title and a no less intimidating picture on its dust-cover, it is an honest journalistic attempt to describe what he saw and what he was told when visiting the country recently.

Perhaps the most interesting chapters in the book are those which deal with his visit to southern Saghalien and with the "floating factories." His narrative also exemplifies a feature of international intercourse by no means exclusive to Japan; there, as in many other countries to-day, every foreign visitor is a potential spy and dangerous accordingly; but every foreign visitor means something extra in the way of the supply of foreign exchange that is in many cases so sorely needed; and Mr. Lajtha's accounts of the careful manner in which his ways were not only guided, but shepherded, reveals how carefully Japan has considered this paradoxical problem.

The book is illustrated by some excellent photographs; but there are some odd transliterations of Japanese words in the text. E. H. DE B.

JAPANESE TRADE AND INDUSTRY—PRESENT AND FUTURE. By Mitsubishi Economic Research Bureau. *London: Macmillan* (*Tokyo: Hosokawa Printing Co.*), 1936. 9×6 *inches;* xviii+664 *pages; map and diagrams.* 21*s*

THE INDUSTRY AND TRADE OF JAPAN. By S. UYEHARA. *London: P. S. King & Son*, 1936. (Second Edition.) 9×5½ *inches;* xii+260 *pages.* 12*s* 6*d*

Ample in scope and treatment, the first of these two able works provides the fullest possible material for those who wish to obtain a detailed picture of the commercial, industrial, and financial life of Japan. Mr. Uyehara's treatise, covering the same ground but less detailed, is intended to be an exposition—primarily intended for foreign readers—of Japan's recent development. Both works alike, dealing with authentic facts, are rigidly objective in method, and criticism of their contents is more a matter for the economist, the international banker, and the man of business than for the general reader. But the contents

of both should interest others: they depict the commercial life of a nation as affected by the prevailing phase of intensive nationalism; and they exemplify the influence of climate and geographical environment on economic and historical development.

It is, for instance, essentially to her climate that Japan, like Great Britain, owes the success of her textile industry; again, like Great Britain, a crowded island country, her people are forced to take to the sea. And there are indeed other aspects in which these two countries are similarly circumstanced: the climate of both is dominated by warm ocean currents and the economic life of both largely reposes on a single industry—coal in the case of Great Britain, silk in that of Japan. There is however one essential disparity between them: if economic forces and national temperament have made Great Britain eccentric, they have made Japan concentric: the one is the head of a loosely-knit imperial organization scattered about the world, the other dominates a highly organized empire immediately adjacent to her, built largely on an elaborate system of modern communications and with the national capital as its sole nerve centre.

Both these works reveal how rapid has been Japan's progress since she re-entered into relations with the rest of the world in the middle of the nineteenth century. It has been the very rapidity of that progress which has been its most remarkable feature. For of itself it represents nothing that is actually new: Japan's seclusion was an historical accident, and what she has done during the past three-quarters of a century is to resume a natural tradition of commerce and navigation interrupted for two hundred years. E. H. DE B.

NEW MAPS OF THE CHINESE PROVINCES. National Geological Survey of China, 1936. *Shun Pao Press, Shanghai.* $7\frac{3}{4} \times 10\frac{1}{2}$ *inches.* 5*s*

The Chinese Geological Survey has again shown its enterprise. It was only in February 1936 that this *Journal* reviewed the large New Atlas of China. They have now issued a third edition of the smaller atlas which first appeared in 1933. The new revision marks an advance on its predecessors in a number of respects. The use of a whiter paper of heavier quality not only makes for greater legibility, but yields a slimmer volume, because both sides of the page now carry print.

China proper is covered by twenty-one topographical maps on a scale of 1/3 M, Heilungkiang and Kukunor being shown on 1/5 M, and Tibet, Sikang and Sinkiang on $1/7\frac{1}{2}$ M. Not only has a definite improvement in the colours of layer-tinting been achieved, but the entire series of maps embodies important additions of detail reduced from the larger "New Atlas." Thus the number of locality names shown has risen from 6000 to 10,000. No changes of political boundaries are made, and innovations in Manchuria are disregarded, the new capital retaining its old name of Chang-chun. The pages of meteorological charts and maps, together with those showing distributions of soil types, arable land and linguistic elements have been entirely redrawn from the latest data. Communications have been brought up to date, and anticipated developments are indicated. One innovation, unique in an altas as far as the reviewer's experience goes, is the presence of a stereoscopic relief map, printed in red and green, which when viewed through bi-coloured celluloid "spectacles" (supplied with the volume) gives a vivid and highly effective impression of a three-dimensional model. The value of this simple device as a step towards the ready interpretation of layer-colouring will certainly not be limited to the Chinese schools and colleges in China for which the atlas was originally planned.

The only concession made to non-Chinese users is the presence of what is termed an "English index" whereby "the maps although printed entirely in

Chinese can be of easy use to foreigners who know how to find the geographical names." The last clause is misleading, for the English index gives only the first syllable of place-names, from which a decimal cross-reference system takes the reader to the appropriate section of the Chinese index where the complete name appears, but in characters only. Thus any one wishing to identify the Wei River must be able to distinguish the correct character from eighteen others also rendered "Wei" before he can use the index co-ordinates to locate it. By allowing two instead of three columns per page in the index, the standard romanized equivalents could have been included, but at a cost of twenty-six extra pages. Evidently the market for this excellent publication is so overwhelmingly Chinese that the publishers did not feel justified in going to this extravagance. From the point of view of European and American geographers however it is a matter of regret, because the handy size and low price would otherwise make it an essential standard volume in the personal library of every student not only of geography but of any aspect of Far Eastern affairs. Even with this drawback the atlas should undoubtedly find a place in all university geographical departments, while sinologists everywhere will welcome it as an authoritative production within the range of any pocket. Its cover bears the honoured name of the late Dr. V. K. Ting, whose tradition is thus ably carried on by his successor, Dr. Wong Wen-hao, and Mr. S. Y. Tseng, the survey cartographer.

G. B. B.

COMPTES-RENDUS DE ONZE ANNÉES (1923–1933) de séjour et d'exploration dans le bassin du fleuve Jaune, du Pai ho et des autres tributaires du Golfe du Pei Tcheu ly. By EMILE LICENT, S.J. (Publications du Musée Hoang ho Pei ho—No. 38.) *Tientsin: "Chihli Press" for Mission de Sienhsien*, 1935–36. $12\frac{1}{2} \times 9\frac{1}{2}$ *inches. Tome I* (1923–1925) 296 *pages. Tome II* (1925–1930) 297–718 *pages. Tome III* (1930–1933) 719–1061 *pages. Tables* 1065–1131 *pages. Illustrations.* Also *Atlas* (21 × 17 *inches*), *containing key-map and* 77 *illustrated route-maps* (scale 1 : 106,000). 600 *francs*

Father Licent has already devoted nearly a quarter century to a study of the North China countryside, and has to his credit a long list of scientific contributions. Although his earlier work was chiefly in the field of botany, his interests have branched out in so many directions that he claims the entire province of natural history as his hunting ground. To inhabitants of Tientsin his name will be permanently associated with the Museum established through his enterprise. To scientists elsewhere he is better known as the co-discoverer with Père Teilhard de Chardin of the important Palaeolithic foyers on the south-eastern edge of the Ordos.

Though Father Licent has written a number of short papers and joint reports describing individual additions to knowledge in the fields of botany, entomology, zoology, palaeontology and geology, he regards himself primarily as an explorer and collector, who travels far and wide, finding new things in unsuspected places, and amassing fresh facts about plant and animal distributions; the evaluation of much of what he finds he can afford to leave to specialists.

The volumes under review follow a similar report on the previous decade (1914–1923), and adopt the same style of diary presentation. They are in fact the transcription of a day-to-day record of field observations, mostly made in slow caravan stages, supplemented by roughly drafted compass traverses and a curiously heterogeneous series of collogravure illustrations, many of which suffer from poor reproduction. The key-map shows a veritable network of journeys covering 15° of longitude and 10° of latitude, besides shorter expeditions in Manchuria. No attempt is made to group together information of the

same kind from different areas; the observations are set down in the sequence of the day's work and occur interspersed with the naturalist's reflections on hospitality received, comments on chance meetings by the way, speculations as to the probable age of indeterminate geological formations, and remarks on the weather, on the behaviour of the caravan, on the cleanness or the reverse of last night's lodgings, and the like. An index of topics is provided, making it more possible to sift from the somewhat rambling account of the day's doings individual sentences bearing on a desired subject. But the items in the index are not sufficiently specific to allow full use to be made of the mass of data scattered through the three thousand quarto pages of diary record which Father Licent will shortly have given us. It is to be hoped that he will find an opportunity some time in the near future to group together by topics, and in more readily assimilable form, the most significant of his conclusions.

Meanwhile it is a commonplace that field records made at the time have a definiteness and a value not always possessed by subsequent writings. Geological sections drawn on the spot, however crude, may be nearer the truth than neatly redrafted diagrams. And the absence of perspective in a diurnal record gives it a vividness that is often lacking from a formal report. The simple account of the discovery of the famous Shui-tung-kou Palaeolithic site—the first of its kind to be recognized in Eastern Asia—bears quotation: "We are off to visit the river banks opposite the village; they are composed of greenish clayey sand with strings of pebbles. At one point Father Teilhard picks out a piece of stone from the bank; it is chipped; another follows; then a whole lot more, with some fragments of bone; there can be no doubt whatever we have landed on a Palaeolithic site. Quartzite is the chief material. In a few minutes we collect a whole series of implements from a layer 5 or 6 centimetres thick; the horizon is extremely abundant. Night falls, and we decide to spend all to-morrow here to study the site more carefully . . ."; and at this point a footnote directs the reader to the report "La Paléolithique de la Chine" by Boule, Breuil, Licent, and Teilhard.

It is unfortunate that the printer uses no distinctive symbol to indicate interpretations or explanatory digressions that are clearly later interpolations not present in the original field record.

The reviewer is familiar with the difficulties of scientific publication in a land where compositors are entirely ignorant of the meaning of the simplest words they are setting up. Errors have a disheartening way of appearing in the galley even after a sixth proof-reading. The author himself draws attention to the fact that all the altitudes recorded on three map sheets in his earlier report are too high by 1400 feet! So one must not be over-critical of occasional lapses. Father Licent is to be congratulated on his achievement in putting into permanent form this intimate picture of the details of his expeditions, pursued with such patience and assiduity almost without intermission for over twenty years. At an age when others would be taking refuge in their armchairs, he continues his explorations in the face of all weathers and under all kinds of political conditions, with his note-book in his hand. G. B. B.

AFRICA

COCONUTS AND CRÉOLES. By J. A. F. Ozanne. *London: Philip Allan & Co.*, 1936. $8\frac{1}{2} \times 5\frac{1}{2}$ *inches;* 336 *pages; illustrations and maps.* 12*s* 6*d*

The would-be settler or tourist should begin this interesting book at Chapter XIII and read to the end. In the last chapter he will find a concise history of the Seychelles. Before he reaches it, he will have revelled in the coral reefs

and coconut palms; he will have met the charming and hospitable créole; he will have tasted all the delights of a fisherman's or painter's paradise. Back in Chapter I there is precise information as to how to reach the Seychelles; and having booked his passage, he would be wiser not to read further until he is well on his way. Otherwise the visitor might change his mind. Hotels, servants, sanitation, and disease are treated in detail, both sides of the canvas being meticulously examined. Mr. Ozanne's description of the state of education and morals casts grave doubts on the achievements of the missionary churches (to one of which he belongs) and of the Educational Department of the Colonial Office during past decades. Lack of adequate education and of the teaching of English leads to unemployment amongst the youth; and if inbreeding and incest are as rife as the author indicates, the effects must surely be serious.

The book is provided with copious geographical notes, a bibliography, an index, and maps. J. P. H. M.

THE CAMBRIDGE HISTORY OF THE BRITISH EMPIRE. General Editors: J. HOLLAND ROSE, A. P. NEWTON, E. A. BENIANS. Volume VIII. South Africa, Rhodesia and the Protectorates. General Editors: A. P. NEWTON, E. A. BENIANS. Advisor in South Africa: ERIC A. WALKER. *Cambridge University Press*, 1936. $9\frac{1}{2} \times 6\frac{1}{2}$ *inches;* xxvi+1006 *pages; maps.* 42*s*

The geographical interest in this record of a most interesting phase in British colonial history is disappointingly restricted, partly because of the ineluctable limits of space and partly because the professional historians' absorption in the political side of their subject has crowded out most of the other and at least equally important and significant episodes of the story. And yet southern Africa, that part of the continent which lies south of the Kunene–Zambezi line, offers unusually rich opportunities to broaden the basis of history by paying more heed to the achievements of the pioneers. It was not unreasonable to expect that the example of the late Dr. J. G. Gubbins might have led to a discarding of historical blinkers in a field so manifestly inviting a freer outlook. If his monumental labours in the field of Africana revealed anything, it was that the development of this vast area owed less to politics and politicians than to the earlier adventurers of all races and creeds who played their parts in the opening up of these romantic lands. Yet what is here given may well serve to clear the ground for the more compendious history when it comes to be written.

Meanwhile there are the first five chapters of this latest volume of the Cambridge History, and scattered passages elsewhere within it, to appeal to the reader particularly interested in historical geography and its allied topics, while the bibliographies are copious—except in respect of early travellers—and serviceably detailed, admirable for such as desire to follow matters further for themselves. Yet the opening chapter, on the geographical environment of South Africa, useful as it may be for those who are unfamiliar with the country, has little direct relation with the episodical story that follows, and reads somewhat uneasily, as if the writer had not been quite sure of its place in the scheme of the volume and had been oppressed beneath the difficulty of dealing with so considerable range of material within so strait a space. Something of the same burden weighs down the final chapter on "Cultural Development," wherein with greater discomfort are huddled certain outstanding themes which, it seems to have been felt, ought to have a place; but what place was very indistinctly envisaged. The result is a hurried scamper over the realms of linguistics, social conditions, science, literature and art, not always without slips, as when on p. 870 T. Baines is confounded with the geologist, A. G. Bain. One cannot

but wish that the editors had boldly taken over the space occupied by both these chapters and given it to the narrative of exploration.

Histories by diverse hands have always the defects of their diversity. The gain in what may be called expertism does not always compensate for the defect of singleness of purpose, the absence of sustained and unified sweep. That there should be a certain amount of overlapping and minor repetitions is inevitable, and it says much for contributors and editors that there is not more. Still there are passages—the present writer found some in connection with the history of education in South Africa—that are not so far harmonized as to save the lay reader from perplexity. Doubtless little inequalities of this sort are part of the price we pay for the benefits of minute specialization in an age of specialists. J. P. R. W.

THE CAPE-TO-CAIRO DREAM: A study in British imperialism. By Lois A. C. Raphael. (Studies in History, Economics and Public Law, edited by the Faculty of Political Science, of Columbia University. No. 422.) *New York: Columbia University Press* (*London: P. S. King & Son,*) 1936. 9×6 *inches;* 514 *pages; illustrations and maps.* £1 0*s* 3*d*

This study of British imperialism in Africa is more limited in scope than its title would indicate, for it deals fully only with the last quarter of the nineteenth century. The Cape-to-Cairo dream did not suddenly fade away about 1900, but its subsequent development is relegated to a rather sketchy epilogue because, as Dr. Raphael says, a complete account from that date "would, indeed, fill another volume." The ground covered is not new, for many aspects of the question have been dealt with in considerable detail in more general works, and especially in W. L. Langer's 'The Diplomacy of Imperialism,' which presumably appeared too late to be included in the bibliography.

Dr. Raphael finds "the birth of the Cape-to-Cairo idea" in the abortive Overland Telegraph proposals of the '70's, in which the Royal Geographical Society showed a special interest and which led to some investigation of the south-to-north route. During the following twenty-five years the conception of rail and telegraphic communication from the Cape to Egypt was merely one aspect of the scramble for Africa, and it is as a force in that scramble that the author considers the Cape-to-Cairo dream. It is a typical story of imperialism, of diplomatic wrangling between the great Powers and of the pressure of big finance; and much of it depended on the pioneer work of some of the most famous of nineteenth-century explorers, whose motives were often purer than the motives of those who came after them. The question is dealt with entirely from the diplomatic standpoint and rightly so, for diplomatic difficulties had first to be overcome before an all-red route from the Cape to Cairo was even a possibility. Indeed it is astonishing how few, except sceptics like Colonel Prout, stopped to consider the physical and economic difficulties in the way of the realization of their dream. Perhaps they thought, as Rhodes did, that money could accomplish anything. Perhaps they were right, but the dream has not been, and may never be, fulfilled.

The story is well told, for Dr. Raphael has handled a complex situation, or set of situations, with considerable skill. Moreover she has viewed the many controversial aspects of British policy with that detachment which, despite its pitfalls, is so often a valuable feature of American work on British history. T. S. W.

NORTH AMERICA

AMÉRIQUE SEPTENTRIONALE. By HENRI BAULIG. États-Unis (Géographie Universelle. Edited by P. VIDAL DE LA BLACHE and L. GALLOIS. Tome XIII. Deuxième partie). *Paris: Armand Colin*, 1936. 11½×8 *inches; 317–639 pages; illustrations and maps.* 90 *fr.*

To write an account of the United States in one volume is no small task. To make it readable as well as informative and reliable is a far harder thing to do. But Professor Baulig appears to have achieved this object, and has produced a most interesting volume. The author is too well known to English readers to need an introduction, but those more familiar with his geomorphological work may be interested to know that he has spent some four years in the United States, and so can write about them from first-hand knowledge. This, combined with his pleasant style and admirable choice of photographs—many of them aerial—allows him to present his material simply and directly.

The volume is based on a regional scheme and the author divides his space carefully. Some regions are treated more fully from an economic and historical standpoint, others from a physical one. Naturally no one book can be exhaustive, and the specialist may find various details omitted or but briefly described. This however is hardly a serious criticism, for the result is a clear synthesis of the geography of a large and important country. It follows an earlier volume which dealt with North America as a whole, and Canada and Newfoundland in detail. The two should be taken together, and they will be found to give a living picture of the continent.

The final chapter of the present volume is entitled "L'équilibre des États-Unis." It deals with many important factors, including the permanent problems of the national life of the States, the population problem, natural resources (*i.e.* apart from their treatment in the regional studies), the recent crisis, industrial statistics, and the Great War and its consequences.

The volume should be of great value as a reference book for the highest forms in schools, and an invaluable aid to junior students at Universities. But it should also be read by the intelligent layman, who will find easily what he wants to know about the States. Too many writers of regional syntheses, no matter how excellent their material may be, fail to make an appeal to this class of reader. Geographical studies may be primarily for the geographer, but the advance of the subject is helped if the knowledgeable tourist or reader meets a volume which not only gives him a true perspective of the country, but also one based on scientific geography. In the reviewer's opinion the volume fully achieves this end. J. A. S.

CENTRAL AND SOUTH AMERICA

LAND OF TO-MORROW: A story of South America. By R. W. THOMPSON. *London: Duckworth*, 1936. 8½×5½ *inches; 460 pages; illustrations and maps.* 16*s*

Mr. Thompson's title is a little misleading, since but two South American countries, Paraguay and Bolivia, were considered, only small sections of these were visited, and their past and present problems, rather than their future, are discussed in the present volume. It has a more serious purpose than a previous book dealing with camp life in Argentina, since it becomes eventually a contribution to the study of the late War of the Chaco.

The author had, originally, no such intention. Wishing, some three years ago, to escape from troubles, he sailed with his wife on a cargo boat that

happened to leave for Buenos Aires; many of the first pages are devoted to minor incidents on shipboard, domesticities, and social happenings in "B.A."; but when Thompson reached Paraguay, then in the second phase of the war with Bolivia, he realized that events of importance were taking place in the woodland heart of the continent, and his journey began to take shape as a mission of investigation of facts and causes. Unfortunately, he had no time for extensive travel, but, after a stay in Asunción, continued upstream to the river port of Concepción, and thence visited a nearby ranch. Penetration into the Chaco proper, and particularly into that section known as the Chaco Boreal, might have earlier brought home to the author the fact—clarified by Sir Christopher Gibson in an excellent study—that wars in South American regions still chiefly populated by native folk are likely, despite politicians, to be settled along ancient racial lines.

Returning to the capital of Argentina after a diversion to the popular falls of Iguassú, Thompson decided to visit La Paz and to discover the Bolivian point of view. He went alone, and succeeding pages are the better for lack of records of domestic conversation approaching banality. The author interviewed various authorities in La Paz, was taken by car to the Yungas valley—a region that, although accessible enough, is off the track of most tourists—and returned with his sympathy for Paraguay crystallized. A "brief summary" at the end of the book of the events of the three-year War of the Chaco is brisk and interesting, but insufficiently documented.

The book is breezy, sometimes very well written, and easy to read; but it would prove of more permanent importance had the author been thoroughly acquainted with the ethnography and geography of Paraguay and Bolivia; and if he had taken pains to avoid the snare of snap judgments and love of rhetorical statements. For a volume with a claim to serious attention, too many errors remain uncorrected; for example, the author remarks on p. 396, with regard to the Madeira–Mamoré cataracts, that "an attempt was made in the 80's to span this distance with a railroad," ignoring the fact of the long existence and operation of the line; on p. 419 he says that Bolivia "lost her port of Arica"; possibly he meant Antofagasta.

Few people who know South America will agree that the recent Chaco war was the "worst calamity in South American history;" with his estimate of the character of Dr. Daniel Salamanca, whom he did not meet; or with the sweeping statement that "Chile holds the key to peace in South America." Faulty proof-reading cannot be responsible for the multiplicity of mistakes in Spanish. Nearly all accents are omitted; "Argentine" is too often printed when "Argentina" should be read; and the author might note that his "algaraba" should be *algarrobo;* "pirhana" should be *piraña* (or *piranha* in Brazil); "ricado" should be *recado;* and that he has mistaken the Spanish-American use of patronymic and matronymic.

The index is fair, although marred by mis-spellings; photographs not first class; maps inadequate. L. E. J.

AUSTRALASIA

PACIFIC RELATIONS: The races and nations of the Pacific area and their problems. By WALTER GAILEY HOFFMANN. *New York and London: McGraw Hill Publishing Co.* (1936). $8 \times 5^{1}_{2}$ *inches;* xx+540 *pages; illustrations and sketch-maps.* 11*s* 6*d*

This work has been designed as a textbook for the higher classes of schools in the United States. Its purpose is "to supply ready access to facts and interpretative material bearing directly on international relations in the Pacific." The

author explains that its "fundamental objectives" are to provide knowledge of the peoples of the Pacific and their problems, to develop the ability to "appraise current events in the light of existing conditions and past events" and the appreciation of "the inter-relatedness of problems, of events, of cultures, of civilization, and of the motives and manner of thought represented in the divergent points of view and in the lives of the different peoples." That is an ambitious scheme for one book, and its value is lessened because the author has attempted too much, for not only does he deal with the Pacific but with the Dutch East Indies, Siam, Indo-China, Russia, China and her dependencies, and Japan. He has collected a mass of information concerning these countries, their inhabitants, and the aims and methods of their governments, but he has not sufficient lucidity to present his material in a form that can be easily digested by the student. The apprehension aroused by the verbiage of his introduction is confirmed by the body of his work. As Macaulay said of Dr. Johnson, his book is written "in a language which nobody hears from his mother or his nurse, in a language in which nobody ever quarrels, or drives bargains or makes love, in a language in which nobody ever thinks." As is inevitable with so much ground to cover, much of his information is so cursory as to be definitely misleading. For example, it is no longer correct to speak of "the headhunting Dyaks," nor are they nomadic, and the Borneo coastal tribes, so far from "learning to cultivate rice," have cultivated it for generations. It is unfair to say that the Arab settlers in the Archipelago use their religion to make money wherever possible, and it is scarcely up to date to give the number of persons who made the pilgrimage to Mecca from the Dutch East Indies in 1927. Neither Sarawak nor North Borneo is a British protectorate: they are protected States which, although it may sound the same thing, is not. The Siamese are dismissed as "rather listless in many ways and prefer farming to other kinds of work." It is ridiculous to speak of the Hakkas as a strange, wild, and savage tribe when any one with a passing acquaintance with them knows them to be the best market gardeners in the Far East. Such points may not be important, but accuracy should be the touchstone of a textbook. In fairness to Mr. Hoffmann it may be said that the arrangement of his material is excellent, his maps are helpful, and his bibliographies (although mainly confined to works published in the United States) will be of use to the student who seeks further information on a particular subject. O. R.

PHYSICAL AND BIOLOGICAL GEOGRAPHY

GREAT EARTHQUAKES. By CHARLES DAVISON. *London: Thomas Murby & Co.*, 1936. $8\frac{1}{2} \times 5\frac{1}{2}$ *inches;* xii + 286 *pages; illustrations and maps.* 17*s* 6*d*

Dr. Davison has already published a separate work on the Japanese earthquake of 1923. In the present volume he gives a more summary account of eighteen other great earthquakes, which are selected for their instructiveness as well as their magnitude. The earliest is the earthquake of Lisbon in 1755, the latest that of Hawke's Bay, New Zealand, in 1931. Six belong to the period 1755 to 1835, the infancy of the science; eleven to the period 1891 to 1931, after the introduction of seismographic instruments. One, the Owens Valley earthquake of 1872, belongs to the intervening period; seismographs were not yet available, but the area affected was examined by a competent geologist two months after the shock. In all but three of the earthquakes the epicentres lay in the great belts of Tertiary mountain-building. The three exceptions are Lisbon (1755), New Madrid on the Mississippi (1811–12), and Cutch (1819).

Our knowledge of the Lisbon earthquake is very incomplete, but there

were many features about it which could not escape attention, and by piecing together the information derived from many sources Dr. Davison has produced a useful and interesting account. It was however the Calabrian earthquakes of 1783 that started scientific investigation of the subject. There were reliable observers in the region at the time and a commission appointed by the Naples Academy examined the area immediately afterwards. Dr. Davison, indeed, remarks that on few earthquakes, even of modern times, do we possess so many and such valuable reports. In the case of the New Madrid earthquake also there were careful observers in the neighbourhood; but its chief interest lies in its marked effects upon the surface, which still persist and were studied in detail at a much later date.

In the chapters devoted to the more modern earthquakes the author's chief difficulty must have been to condense into a limited space the material available; but the Alaskan earthquake of 1899 was exceptional. It was recorded by seismographs all over the world and its position approximately determined. But its epicentre was in an almost uninhabited region, and it was not till six years later that the remarkable uplifts and depressions which accompanied the earthquake were examined.

Each of the eighteen earthquakes has features of special interest which Dr. Davison brings out very clearly, and each chapter is followed by a full bibliography. The reader will particularly appreciate the care with which he has collected and collated the scattered notices of the less well-known earthquakes. He does not neglect unexplained phenomena, such as visible ground-waves, about which some seismologists are very sceptical. In face of the accounts of many eye-witnesses in varying circumstances the ordinary man will find it hard to believe that they are entirely subjective; it is easier to think that seismologists do not know everything about earthquakes, especially about their effects on loose superficial material.

The book is illustrated by 122 maps and diagrams in the text and 12 plates of photographs, and it is provided with a good index. It can be strongly recommended to all those who are interested in earthquakes. P. L.

GEOGRAPHISCHE KAUSALITÄT. By E. MARKUS. (Acta et Commentationes Universitatis Tartuensis (Dorpatensis) A XXX$_5$.) *Tartu*, 1936. 9½×6½ *inches; 202 pages; maps and diagrams*

This work is avowedly methodological. To the author, whose thought is entirely German, geographical research aims at the analysis and interpretation of facts of the "geographical landscape," and the delimitation of regions. By besprinkling the text with philosophical terms, some the author's own, *e.g.* chorogenesis (*Chorogenese*), the first impression given to the depressed foreign reviewer is of a formidable, closely reasoned, highly theoretical treatise.

When read through, it resolves into something different. The first chapter treats of the "Concept of Cause in Logic . . . and Geography," and is a succession of quotations from, or summaries of, writers on scientific methodology (an excursion is even made as far afield as Quantum Mechanics), which are brought into not very effective contact with Humboldt, Ritter, and Hettner. The body of the work is a long succession of short regional studies, drawn from all over the world, and from many sources, mostly German. The author's own detailed researches into the Plant Geography of Baltic countries have been very freely drawn upon; but we find also a rather bald summary, for instance, of Sten de Geer's cartographic delimitation of Balto-Scandia. The design is to take the reader from local and simple cases (of "geographical landscapes") to world-wide and complex ones; to illustrate various types of regions, and the

nature of regional boundaries. Little, then, is new, save the arrangement and terminology, and much matter would ordinarily enter an elementary course on general physical and human geography. It is, in fact, clear that one tendency of the German mind has been well displayed: the pretence that the invention of a new label illuminates a whole subject. It is really a little distressing to read a plain account of an oasis in the Tarim Basin under the heading of "Autogenic Course of Development" (*Autogener Entwicklungsgang*).

The book is likely to be most useful to students at Tartu, and perhaps at other German-speaking universities. Indeed, the simple style, the abundance of clear diagrams and tabular summaries, bespeak close connection with the lecture hall. The English reader will find little of profit, save the summary of Dr. Markus' own original work, mostly hidden in remote German, Lettish, and Russian publications. J. H. G. L.

CARTOGRAPHY

MAP OF ROMAN PALESTINE. By M. Avi-Yonah. (Reprinted from "The Quarterly of the Department of Antiquities in Palestine," vol. V.) [*London: Humphrey Milford*, 1936.] 11×8½ *inches;* 139–193 [55] *pages. Map.* 3*s*

The publication of this map is not unconnected with the compilation of Sheet H.36 (Cairo) of the International Map of the Roman Empire. On his map Mr. Avi-Yonah has adopted the Conventional Signs of the larger scheme "with certain amplifications, justified, it is hoped, by the larger scale of the map." There is no end to the multiplication of the conventional signs possible on historical maps. Those shown on any given map must necessarily represent a balance between one extreme and another, and the selection of the signs must be determined by the purpose of the map itself.

Broadly speaking, archaeological maps may be divided into two kinds—index maps and those which attempt to portray the political and social organization, road system, and so forth of a given area at a given period. Multiplicity of signs is permissible (when necessary) on index maps, but not on others. The whole question was debated at an early stage in the organization of the International Map of the Roman Empire. There was a tendency in certain quarters to regard the map as an index of ancient monuments and to ignore the other aspect. Fortunately this tendency was not approved by the majority.

While admittedly there is room for more symbols on a map four times the scale of the Million map, we still think that, for instance, to show seven symbols in a line under the name of Caesarea is excessive. There is one symbol in particular which merely adds to the confusion, and that is the circle for amphitheatres. For this a letter O has been adopted, which is quite indistinguishable from the capital letters of some of the names on the map.

For the rest however we can only praise the map and rejoice that this small but important province of the Roman Empire should at last be able to be comprehended at a single glance. We must naturally be grateful to the Director of Surveys for giving us a map at all: it is not every Survey Department which could find time to produce archaeological maps in the middle of a serious rebellion; and the map is cartographically adequate, even if it cannot be called beautiful.

The problem of printing a satisfactory combination of names written in different styles in a crowded region like that round Jerusalem is one that would try the ingenuity of the most skilful draughtsman. It is not one that can be easily solved, if at all, when stamping is the method adopted.

Nevertheless, we only wish it were possible to improve the styles of letters used for stamping names on maps. Perhaps readers of this *Journal* are rather spoilt in this respect by the standard achieved in the maps that have been published here during the last few years. The present reviewer would merely throw out the suggestion that Survey Departments in all countries might well overhaul their old stamping outfits and bring them more into line with the requirements of modern taste. O. G. S. C.

ECONOMIC AND HISTORICAL GEOGRAPHY

DIE VORHERRSCHAFT DER WEISSEN RASSE. By WAHRHOLD DRASCHER. *Stuttgart und Berlin: Deutsche Verlags-Anstalt*, 1936. 9×6 *inches;* xii+386 *pages*. *M*.9

The diffusion of European culture throughout the world has been the theme of numerous studies, historical and geographical. Herr Drascher's work is not designed to compete with these; it is not a detailed chronological or regional study of this movement, but a general critical consideration, to illustrate a particular outlook on world history. The reader will not require to read many pages before the inspiration of the book becomes clear to him. The author confines the term "white race" to the seafaring peoples of western Europe. Their achievement of world domination is regarded as a spiritual and material victory over human opposition and the forces of nature, exemplified as much in scientific invention as in territorial acquisitions. The nineteenth century saw the zenith of their influence, when under their leadership the world became an economic unit, based largely upon the subjection of the "coloured" races.

The passing of this state of affairs is analysed in considerable detail. His conclusions, though vigorously stated, are not unusual, and are mainly based upon the effects of the Great War—the ill-balanced development of the nations of Western Europe, resulting in a large population severed from the land; the rise of non-European nations to various degrees of economic independence; the shortcomings of the colonial powers in compromising with their native subjects and in encouraging the growth of rivals such as Japan; the dissemination of false ideas of their culture through the cinema; the spread of subversive doctrines from Russia. France is censured for her attitude towards her colonial subjects; England for failing to see in 1914 that Germany was only one, and not the most dangerous, of the rivals to her Empire, and for promoting democratic institutions in India. Though he states no remedy, in so many words, his solution is plain: a return to stricter racial differentiation, the restoration of the superiority of the European in relation to the native, and the combating of liberal thought. The means of achieving this reformation are clearly designated. Germany is ready to lead the "white" races back to their former position of world-wide authority. To read this rather forbidding book, with its unbroken pages of heavy Gothic characters, is to realize how subtly political theories can be presented as ostensibly a serious contribution to knowledge. G. R. C.

AN INTRODUCTION TO THE BIBLIOGRAPHY OF CAPTAIN James Cook, R.N. By MAURICE HOLMES. *London: Francis Edwards*, 1936. 9×5½ *inches;* 60 *pages*. 10*s* 6*d*

This useful guide consists of a list of the more prominent men who served under Captain Cook, with short notes on their careers; an annotated list of titles and collations of the primary printed sources for Cook's voyages; and two short lists of the less important contemporary publications and modern works on the circumnavigator. As far as the present writer has been able to check them, the

bibliographical details have been accurately and thoroughly compiled. The annotations to the narratives of the voyages disentangle the complications of the numerous anonymous narratives, published to forestall the official accounts despite the vigilance of the Admiralty, and the bickerings in which Dalrymple, Hawkesworth, the Forsters, and Parkinson, among others, became involved. Thus the book has a wider interest than the title conveys, and will assist the student of Captain Cook and his achievements to obtain a true perspective before engaging in more detailed studies of the literature. It should be noted that the compiler, in his own words, has left "the very difficult and complicated business of Cook cartography . . . severely alone."

A definitive edition of Cook's voyages is much wanted, but this awaits the listing and critical examination of all the relevant material printed, manuscript, and cartographic. Mr. Holmes has made a beginning with this work, and it may be hoped that his example will prompt others to continue with it. G.R.C.

CAPTAIN JAMES COOK. By Vice-Admiral GORDON CAMPBELL. *London: Hodder and Stoughton*, 1936. 9×6 *inches;* 320 *pages; illustrations and charts.* 15*s*

Admiral Campbell has written a straightforward life of Captain Cook, which does not profess to add any new facts to the available voluminous material, but which is valuable as a portrait of a great navigator by a sailor of distinction. The emphasis therefore is on the conditions of naval service in the eighteenth century and Cook's relations with his officers and crew. Though the author is occasionally at pains to defend Cook against charges of severity, his analysis of his character is by no means partial. He selects as Cook's outstanding characteristics his "unremitting perseverance" and his modesty, quoting at length the interesting and little-known appreciation by the German sailor, Zimmerman, which is perhaps the most vivid sketch of the circumnavigator's personality by a contemporary that has survived.

The book is clearly written and should serve its purpose as an introduction to its subject. There are one or two minor errors, *e.g.* on p. 15, the *Trinidad* is given as the name of the sole ship of Magellan's fleet to complete the circumnavigation, an error for the *Victoria;* also the date of Drake's voyage is incorrectly stated. The paraphrase of the accounts is not always accurate. Though no bibliographical references are given, a useful list of "Logs and Journals" is included. There are a number of clear maps. G. R. C.

WILL MARINER: A true record of adventure. By Vice-Admiral BOYLE TOWNSHEND SOMERVILLE. *London: Faber and Faber*, 1936. 8½×5 *inches;* 370 *pages; illustrations and charts.* 10*s* 6*d*

Mariner's account of his extraordinary stay on Tonga has long been a Pacific classic; and it has long been available only in the 1816 edition edited by Dr. Martin (republished 1827). Admiral Somerville has done a service in rewriting the book in the pattern of authentic and readable adventure. This book, completed by his sister, Miss E. OE. Somerville, after his murder, is a fitting memorial both to the original author and his modern interpreter, each of whom were bold men of the sea.

The first part of the book is an account of the voyage of the privateer *Port-au-Prince*, on which Will Mariner sailed as a lad of thirteen. It is a most exciting story, complete in itself, leading up to climax in the terrible massacre of the ship's crew by the Tongans. Will, now a lad of fifteen, escaped by good luck and his personal charm. He was adopted into the chiefly class of this aristocratic, non-cannibal society. His account of all that he saw is fascinating. It

shows how a simple uneducated lad can adapt himself to a strange society and prosper in it. It shows how the coming of guns broke up the old ethics of personal status and warfare, and describes in detail the way in which the ship's cannon and the willing white survivors entered into the native struggles, with awful results.

The exacting student will still need to refer back to Mariner's original account for certain points. But the present volume can be recommended to all who are interested in adventure or anthropology. In particular, every one should consult the passages on pp. 182, 217, and 240, which give the profound Tongan views on our systems of writing, money, and munitions. T. H. H.

GENERAL

ELIZABETHAN SEAMEN. By DOUGLAS BELL. *London: Longmans, Green* (1936). 9 × 5½ *inches;* x + 324 *pages; illustrations and maps.* 12*s* 6*d*

This book gives a most attractive and inspiring account of the deeds of our seamen and merchants of the sixteenth century, and of the hardships which they overcame. Many of the voyages described are well known, but others, such as the expeditions of Sir Hugh Willoughby, and later of Richard Chancellor, into Russia, are by no means as well known as they deserve. The very modern language employed may occasionally shock the reader, but Mr. Douglas Bell's view is that "It is not desired to give an impression of 'quaintness.' Elizabethan seamen were not in the least 'quaint,' but virile men remarkably like the men of to-day." An interesting outlook, but one which tends to hide from the uninstructed reader the great difficulties in all material matters under which Elizabethan seamen laboured.

The illustrations are unusually interesting. Where individuals are concerned, Mr. Douglas Bell has confined himself to photographs of good contemporary pictures. For ships, since there are no contemporary models in existence, nor any reliable pictures of the individual ships, the author provides photographs of the best modern reconstructions available, and these serve admirably to illustrate the very interesting description of Elizabethan ships and their construction which is contained in the last chapter. G. S. L. C.

THE MONTHLY RECORD

THE KING'S ACCESSION AND ROYAL PATRONAGE

In the first days of His Majesty's reign the Keeper of the Privy Purse was commanded by King George the Sixth to state that His Majesty was pleased to intimate to those Societies and Institutions which had been granted Patronage by King Edward the Eighth that they might continue to show the Sovereign as their Patron during the present Reign.

At their meeting on 11 January 1937 the Council resolved upon this Address to His Majesty:

THE LOYAL ADDRESS OF THE ROYAL GEOGRAPHICAL SOCIETY ON THE ACCESSION OF HIS MAJESTY KING GEORGE THE SIXTH

MAY IT PLEASE YOUR MAJESTY

We the President Council and Fellows of the Royal Geographical Society with our humble duty to Your Majesty beg to assure You of our most loyal devotion to Your Person on Your Accession to the Throne of Your Ancestors.

We pray Your Majesty to accept our dutiful and grateful thanks for Your Royal Patronage which You have already been graciously pleased to signify will be extended to our Society, and we promise to do our utmost to be worthy of it by advancing those objects for which our Society was founded by His Majesty King William the Fourth and to which it was dedicated anew when You in the name of Your Royal Father inaugurated our Centenary celebrations six years ago.

We pray for Your Majesty and for Her Majesty the Queen long life and happiness in the abiding affection of Your People and in the prosperity of the Realm.

At the House of the Society this eleventh day of January in the first year of Your Reign.

HENRY BALFOUR
President

W. L. SCLATER
Honorary Secretary

ARTHUR R. HINKS
Secretary

which having been engrossed upon vellum and sealed was conveyed to the Secretary of State for Home Affairs for submission to His Majesty.

On February 15 the Home Secretary informed the President that the Loyal Address of the Society had been laid before the King and that he was commanded to convey to the President His Majesty's deep appreciation of the sentiments of loyalty and devotion to which it gave expression.

Further, the Keeper of the Privy Purse has informed the President that His Majesty will be graciously pleased to continue the grant of the two Gold Medals which have been awarded annually by the Sovereign: the Patron's Medal bearing His Majesty's effigy and the Founder's Medal that of King William the Fourth. The President has expressed in their name the thanks of the Society for His Majesty's gracious favour.

AWARD OF THE GEOGRAPHICAL ESSAY PRIZE

In 1933 the Council of the Society resolved to offer a prize of Ten Pounds annually for an Essay of not more than 5000 words: either a critical survey of recent additions to knowledge in a limited geographical field selected by the writer, or a discussion of some current problem in its geographical aspects. Competitors for the prize must be registered Internal or External Students reading for Honours in Geography at one of the Universities of Great Britain and Northern Ireland during the academic year within which they submit their essay. Essays must be submitted during the month of December, the award is made not later than March 31, and the Prize Essay is published in the *Geographical Journal*. Photographs, sketch-maps, and diagrams should be restricted to the minimum essential to illustrate the essay.

Eleven essays were submitted in December 1936, and the prize was awarded to Mr. W. G. V. Balchin, student of St. Catharine's College, Cambridge, for his essay on "The erosion surfaces of North Cornwall," which will be published in a forthcoming number of the *Journal*. Essays in competition for the fifth award of the prize should be addressed to the Secretary of the Society by 31 December 1937.

THE POPULATION OF BERLIN-BRANDENBURG

A detailed study of population changes in Berlin and Brandenburg during the period 1875–1925 is given by Herr G. Deissmann in No. 11 of the *Berliner Geographische Arbeiten*, which brings out the extent of the exodus from the rural areas and the concentration of population in Greater Berlin. An appendix gives the population of all cities, districts and estates for the years 1875, 1890, 1910, and 1925, and the main features of growth and decline are shown on two folding maps. The contrast between city and countryside is of course very strong. In the period under consideration, the city population had increased from 58 to 78 per cent. of the total, while the proportion of agricultural workers had declined from 33 to 13 per cent. The number of those engaged in trade and commerce increased sevenfold. The growth of Berlin was largely due to migrants from Brandenburg and the East Elbe area, who are estimated to have numbered about two million. From 1915 to the end of the period there was no natural increase of population in Berlin.

The decline of the countryside, consequent upon the development of Germany as an exporter of manufactures, overseas competition in agricultural products, and the introduction of mechanical appliances, is reflected in the fact that 40 per cent. of all the Landgemeinde had attained their maximum population by 1875, and a further 21 per cent. by 1891. The areas of greater depopulation appear to lie in the marshlands of the Oder, Warthe, and Netze, in Northern Havelland, south-west Kuppin, and Sorau. Political changes have also had their influence, as some of the smaller towns lost their markets when Poland became a sovereign state. It is also recorded that by 1925, 153,000 inhabitants of the ceded territory had migrated to Berlin-Brandenburg.

THE CARTOGRAPHICAL WORK OF GRAZIOSO BENINCASA

One of the most prolific of fifteenth-century cartographers was Grazioso Benincasa of Ancona, and frequent references to his work have been made by students of portolan charts. Up to the present however no critical list of his charts and atlases has been compiled. This has now been done by Signora Marina Emiliani in an article in the *Bolletino* of the Italian Geographical Society (August–September 1936), in which she also gives the little that can be gathered from documents about his career. His charts and atlases are scattered

throughout the great libraries of Europe, thus making it difficult for commentators to avoid confusing individual items, particularly when they have previously been in the hands of private owners, and part of the value of the author's work lies in the clearing up of the mistakes of her predecessors.

Benincasa was born at Ancona in the first quarter of the fifteenth century. His early years were spent on commercial voyages in the Adriatic, the Mediterranean, and the Black Sea. Manuscript notes of his, dating from the period 1435–45, have survived. These consist of miscellaneous material which probably served him when he began his career as a chart-maker. The occasion of his migration to Genoa in 1460 and adoption of this profession arose from financial difficulties following upon the seizing of his vessel by Genoese pirates off Tunis. It does not appear whether he was successful in obtaining compensation for his losses in Genoa, but his first chart is dated the following year at that city. In 1462 he went to Venice, where, with certain intervals, he was at work until 1474. The later years of his life were spent in his native city, Ancona. The year of his death is not known; his last chart however is dated 1482. In all there are seventeen atlases and four charts dated and signed by him, and numerous others attributed to him or to his school. The author lists these with full particulars and notes. Benincasa's work, except for the representation of one area, does not appear to have developed pronouncedly during the years of his activity, and the content and make-up of his atlases varied little. The exception is the delineation of the north-west African coast-line. In the atlas of 1465 it was extended as far south as Cape Verde, and in one of 1468 to Cape Mesurado, the Cape Verde Islands appearing for the first time on any map. The information on which he based this extension was probably obtained from the Venetian traveller Cadamosto. The author also describes for the first time in detail an atlas in the former library of the Seraglio in İstanbul, which is accepted as the work of Benincasa. It is unique among his atlases however in including a general map of the world after Ptolemy, and a general map of Europe and the Mediterranean. His son Andrea, who took part in the official life of Ancona, was also the author of two charts and one atlas.

CLIMATIC CHANGE IN THE ALPS

A paper by the Italian glaciologist Umberto Mónterin, entitled "Il Clima sulle Alpi ha mutato in Epoca Storica "? is published by the Comitato Nazionale per la Geografia (Bologna 1937) and also appears as No. 16 of the *Bollettino del Comitato Glaciologico Italiano*. It supports the conclusions of H. Kinzl, which were published in the *Zeitschrift für Gletscherkunde* (Bd. XX, 1932), that between the middle of the sixteenth and the middle of the nineteenth centuries development of the Alpine glaciers was the greatest known within the historic period. The author strengthens Kinzler's testimony with reference more especially to evidence afforded by ancient irrigation channels and by the upper limit of woodland in the Aosta and other valleys on the Italian side of the Alps.

Since the middle of last century there has been a general retreat of the Alpine glaciers, which latterly has been accelerated, and the question is asked whether we are about to witness a return of the warm, dry phase of climate which the chronicles indicate to have prevailed before the sixteenth century. The alpine pastures, say the mountaineers to-day, are " going dry " and do not yield such good returns as formerly.

Rejecting the argument that the effects observed can, at any rate in the main, be attributed to human factors, Mónterin sums up the evidence for milder and drier climatic conditions in the Alps before the middle of the sixteenth century than subsequently as follows: (1) the upper limit of woodland was

higher and became successively lower; (2) the network of irrigation channels was more extensive and gradually went out of use; (3) transit across the elevated valleys was easier, many of the routes being abandoned later; (4) the glaciers were smaller on account of a higher snow-line.

It is of interest to note that the period of heavy precipitation in the Alps coincides very well with that of high water-level of the Caspian Sea (*Geogr. J.* 88 (1936) 476). Reference is made in the paper to recent retreat of the glaciers in Iceland and other regions. Reports of glacial retreat in different parts of the world have of late years become, in fact, numerous, and it is to be hoped that glaciologists will attempt to co-ordinate the evidence and indicate the probable trend of events.

TRADE BETWEEN THE ROMAN EMPIRE AND THE FREE GERMANS

Miss Olwen Brogan summarizes the archaeological evidence for trade between the Roman Empire and the Germanic tribes east of the Rhine from the reign of Augustus to A.D. 400 in the *Journal of Roman Studies* (vol. 16 (1936) 195–222). Primitive communications within this area existed from very early times, and tracks, causeways, and perhaps bridges can be traced, while an important part was played by the rivers. In the north-west numerous finds have been made on the Frisian coast, and it is probable that Roman traders had reached as far as the mouth of the Elbe. A well-defined route ran across the south of the Jutland peninsula, and a feature of the distribution maps is a concentration of finds in the island of Zealand. In western Germany, the route along the Lippe, leading to Minden on the Weser, was important: a parallel route, the "Hellweg," ran from Asciburgium on the Rhine to Paderborn, whence imports were disseminated as far as the line of the Elbe. South of the Main few Roman objects have yet been found. Trade routes ran north from various points on the Danube, *e.g.* the route from Vienna to Central Bohemia, an area which fell markedly under Roman cultural influences. The most important route here however was that from Carnuntum to the Eastern Baltic, which followed closely the line of the prehistoric amber route. Along this line, particularly in Upper Silesia and also in Pomerania, Roman finds have been numerous. The so-called "Varangian" route of later times, *via* the Vistula to the Black Sea, does not appear to have been much used.

Among the types of objects found, money was widely distributed, even before the period of large-scale payments to the Germanic chiefs. Republican coins and coins of the early Empire are abundant in Western Germany, and the troubles of the third century apparently affected this area little, for coins of the Gallic emperors are found up to and beyond the Vistula. Some bronze vessels of the later Republic are found, mostly from Aquileia over the Eastern Alps; in the first century Campanian wares arrived by way of the Danube, and in the following century Gallic wares, probably coming from Lyons. The Germans were skilful metal-workers, and there is evidence for metal industries at Eisenburg in the Palatinate and in the neighbourhood of Aachen. Pottery of Roman provenance was also largely imported, and specimens of "terra sigillata" have been found on one hundred sites, particularly from the second century around Minden and on the lower Rhine. Glass, glass beads, bronze statuettes, and brooches also figure among the finds. It is difficult to determine what the Germans exported in exchange—probably cattle, grain, and other agricultural produce. Amber also figured among the exports in the Imperial period. It is probable however that the Germans obtained their supplies of Roman goods very largely in exchange for the large sums of money paid to them to keep the peace.

THE ACCIDENT TO THE O.U. GREENLAND EXPEDITION, 1935

A communication has been received, through the Foreign Office and His Majesty's Minister at Copenhagen, from the District Officer of the Sukkertoppen district of Greenland reporting the discovery of a body presumed to be that of Mr. Michael Atter, who was drowned in the River Safartok on 17 August 1935, while with the Oxford University Greenland Expedition. The body was found by three trappers, who brought away a boot which was subsequently identified by Mr. Peter Mott as having belonged to Mr. Atter. His relatives and friends will be glad to know that the body has been buried with care in the neighbourhood of the spot where it was found, Kangimuk Sangmissok on the Stromfjord, about three Danish miles from the place where the accident occurred (*Geogr. J.* 88 (1936) 157).

A RURAL ECONOMIC SURVEY OF SIAM

We have received from the Peabody Museum, Harvard University, a copy of 'Siam: Second Rural Economic Survey, 1934–5,' by Mr. J. M. Andrews. This study is based upon a series of domestic budgets of cash income and expenditure obtained from numerous households in all the chief agricultural divisions of Siam. Though it is obvious that money payments will differ in importance in the various areas, and that the figures obtained by this method will not be strictly comparable among themselves, the study affords a basis for a general understanding of Siam's rural economy. The survey was largely prompted by the economic depression, and contains suggestions for the future development of the country. The main support of the population is of course agriculture. In all areas except the south, rice is the dominant crop: in the north and north-east this is mostly a glutinous kind unsuitable for export. In the centre, non-glutinous rice is grown for export on a highly commercialized system, and in some parts of this area there is an almost equal specialization in certain fruits. In general, wherever glutinous rice can be grown, the non-glutinous variety can be introduced with success; if however the export of rice is to be developed, it is urged that the rice must be standardized, and exported direct from Bangkok, to avoid adulteration by middlemen.

In Southern Siam, vegetables and fruit are obtainable with little cultivation, so that the population is not obliged to work as strenuously as elsewhere: low prices have also temporarily destroyed the copra industry. It is thought that rubber plantations could be successfully established here.

Specialization in various vegetables and fruits has been successful in most areas, but future development must depend upon the improvement of communications. The possibilities suggested are cotton in the north-east and tobacco in the higher valleys in the north. In the coastal regions of the south-east, pepper would become of even greater importance if a systematic effort were made to eradicate the fungus disease. The native fisheries also deserve attention, for at present a considerable quantity of tinned fish is imported. The Report examines several other aspects of Siam's internal economy, including agricultural indebtedness and co-operative societies.

TROPICAL PLANTS AND WORLD TRADE

We have received from the publishers the first three volumes of Dr. Andreas Sprecher's 'Tropische und subtropische Weltwirtschaftspflanzen,' III. Teil. (Stuttgart: Ferdinand Enke, 1936). These deal with the "luxury" products of the tropics which form important items in world trade—coffee, cocoa, and tea, with the allied plants of more restricted use, guaraná, kola, and maté. In each case the author gives an historical account and a systematic description of the

plant, the geographical conditions which favour its growth, the organization of the plantations, the uses and distribution through the world of its products, and a sketch of the economics of the industry, with statistics which are in many cases no later than 1931. These monographs form useful sources for much information otherwise difficult to find. They have a number of illustrations and diagrams, but no attempt has been made to map in any detail the distributions of the producing areas.

In the case of cocoa and coffee, plants originating in one continent have flourished best as an item in world trade in another, cocoa having been introduced from South America into Africa, and coffee from Africa to South America, especially Brazil. The Gold Coast still retains its place as the most important exporter of cocoa beans, but the author considers that, with the ageing of the trees, first introduced in 1879, this position will be challenged. The West Coast of Africa however is likely to maintain its position, for the most striking development of recent years has been the rise of Nigeria to third place among the world-producing centres.

INTERNATIONAL BIBLIOGRAPHY OF GEODESY

The Association of Geodesy of the International Geodetic Union has been occupied for some years with the task of organizing an international bibliography of geodesy. The first volume of this work—'Bibliographie géodesique internationale,' edited by General Georges Perrier and M. Pierre Tardi, has now been published by the Secretariat of the Association (Paris: 19 rue Auber) at the price of 185 francs. This volume covers the publications of the years 1928 to 1930, each volume being designed to cover the three years which normally elapse between the General Assemblies of the Union. The main divisions of the classification employed are general geodesy, mathematical geodesy, geodetic and positional astronomy, physical geodesy, figure of the Earth, and constants and properties of the Earth as a whole. In the compilation of the 1400 entries, thirty-five geodesists of many nations have taken part; articles in five hundred periodical publications and official reports, in addition to books, have been analyzed. Short précis of the more important items are given in French, and an author index is provided. The work of compilation and editing appears to have been carefully done. The format is perhaps somewhat too large for the rapid handling of a work of this nature, but it has been selected to correspond to that of the *Travaux* of the Association. There can be no doubt of the value of such a publication to the student of geodesy, and this value will be increased if volumes 2 and 3, which are in preparation and will bring the bibliography up to 1937, can be published without much delay.

OBITUARY

SIR PERCY ZACHARIAH COX, G.C.M.G., G.C.I.E., K.C.S.I.

We record with deep regret that Sir Percy Cox, until lately President of the Society, died on 20 February 1937 in the hunting field near Bedford.

At the opening of the evening meeting on February 22 Dr. T. G. Longstaff, Vice-President in the Chair, announced the grievous loss which the Society had sustained, and said:

"This is not the occasion to dwell on the great achievements of a life spent in the service of the State. Sir Percy Cox's name will go down in

history as the greatest personality of his generation in the tale of British influence and achievement in the Middle East. His best-known geographical work relates to the hinterland of Muscat, culminating in his memorable journey across the Murrandan peninsula. He also did more than any one else to elucidate the geography, geology, and history of the islands of the Persian Gulf. It was thanks to his support—for he had the confidence of the Arabs as has no man before or since—that Cheesman, Philby, and Thomas were able to make their historic journeys. He was a great authority on the birds of Arabia, Mesopotamia, and the Gulf, and enriched the Zoological Gardens by the gifts of rare animals such as the oryx (the unicorn) and the wild ostrich.

"He was a great horseman. We can snatch some comfort from the fact that he died in the saddle, as he would have wished.

"I ask you to endorse the action of your Council in sending to Lady Cox, his constant companion through thirty years of arduous work in trying climates, a message conveying our sincere sympathy with her, our own deep sense of loss, and our profound appreciation of the qualities of her late husband."

MEETINGS: SESSION 1936–37

Sixth Evening Meeting, 25 January 1937. The President in the Chair

Elections: Miss Grace Armstrong, B.A.; Louis St. Clair Bartholomew; Kenneth Vaughan Beach; Lt.-Col. Charles Bramhall, O.B.E., R.A., M.C.; Lt.-Col. Henry Robert Brown, I.M.S.; Lieut. A. J. Cobham, R.N.; Kenneth Gandar Dower; Edgar Charles Hay-Ellis, M.A.; Captain N. Eustace, I.A.; James Farrell; J. H. Furbay, PH.D.; Norman Herd; Geoffrey Seccombe Hett, M.B., F.R.C.S., F.Z.S.; August Hoette; Miss Ethel E. Hooper; Miss M. D. M. Lea, B.A.; Rev. Philip Calvert Lindsay; Sir Ewen Reginald Logan, K.C.M.G.; Mrs. Dorothy Mary Meriel Lukin; Mrs. Violet Capel McKay; Captain Donald MacMillan; Arthur Holte Macpherson; H. P. Manton; Captain Alan Ernest Monypenny; Rev. Herbert Osborne; Miss Dorothy A. Pain; Leonard Charles Alfred Parkin; Mrs. Ella Marie Phillips; John Vincent Phipps; G. Lloyd Rees; William Ruthven, B.SC.; William Salisbury; Henry Frank Sutton; Mrs. Caroline Mabel Wade; Miss Eleanor C. Watterson; Stanley Charles Wentworth Wilson

Paper: Northeast Land 1935–1936. By Mr. A. R. Glen

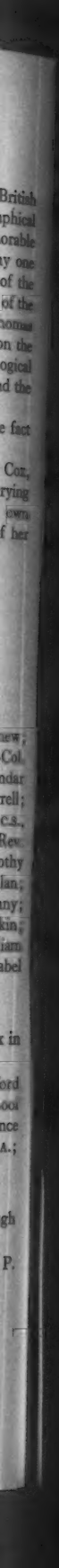
Seventh Evening Meeting, 8 February 1937. Major-General Sir Percy Cox in the Chair

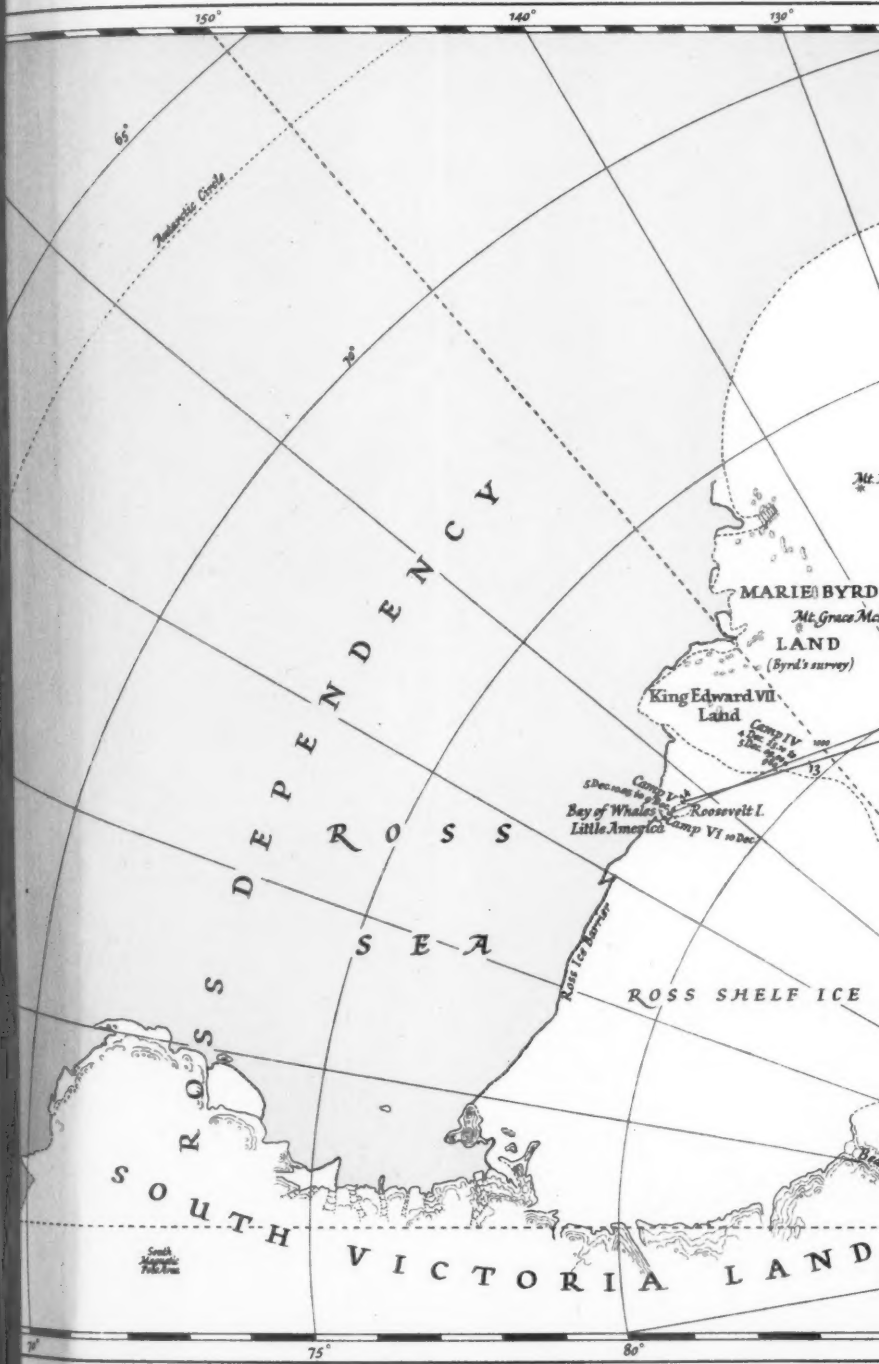
Elections: Patrick Gerard Brennan, B.SC.; James Rennard Dean; Clifford Thomas Dolling, B.SC.; Captain Guy Hiddingh; V. D. Kuppusamy; Looi Leng Yim; William Henry Parker, B.A.; W. F. Schneeberger, D.SC.; Laurence George Cecil Sims, B.A.; Captain Major Bristow Skinner; O. H. K. Spate, B.A.; Hugh Reginald Taylor

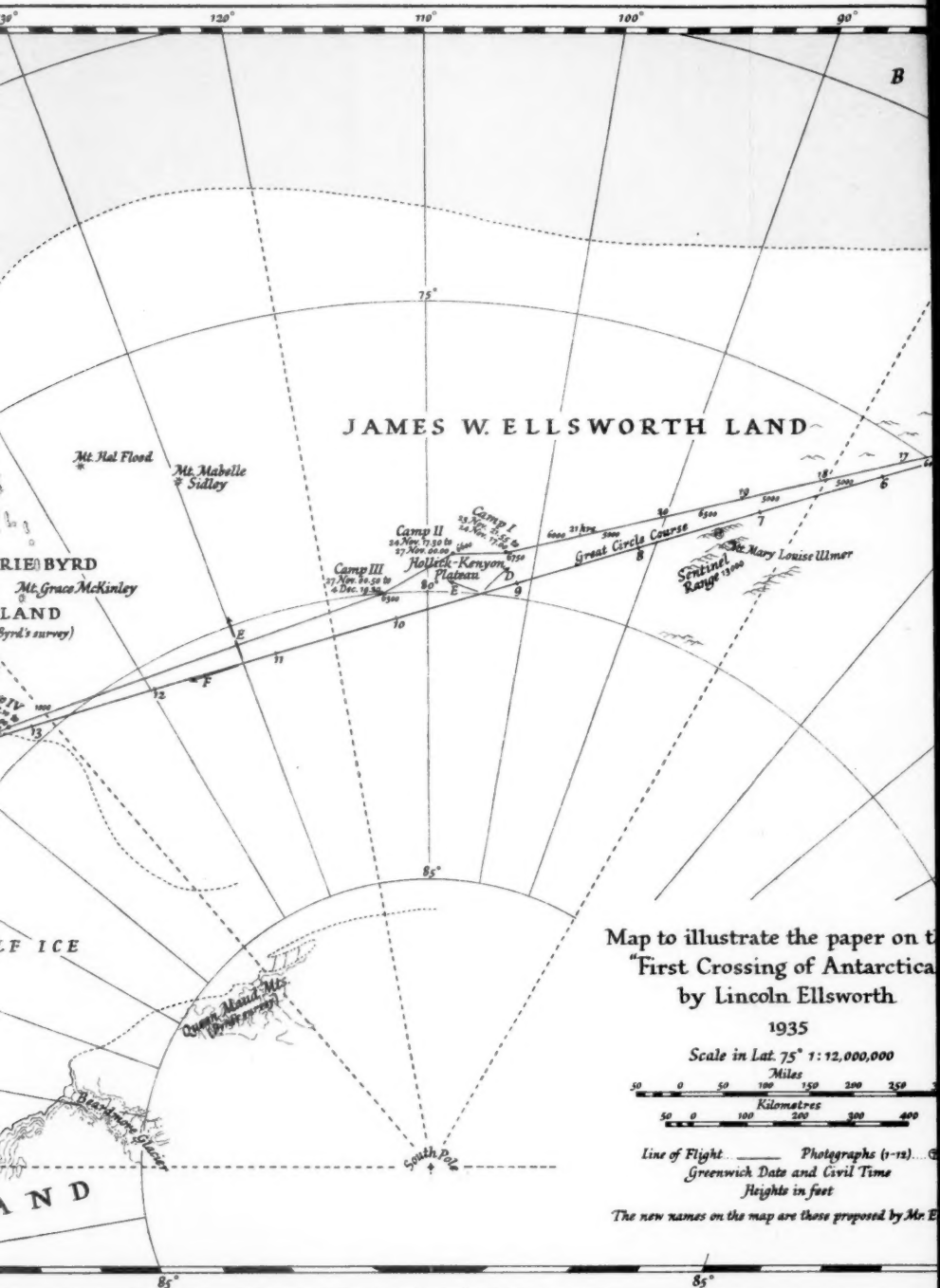
Paper: A Volcano under an Ice-cap. By Dr. Niels Nielsen

Fifth Afternoon Meeting, 15 February 1937. Admiral Sir William Goodenough in the Chair

Paper: The Economic Development of the Soviet Arctic. By Mr. H. P. Smolka




150°
140°
130°
65°
Antarctic Circle
ROSS DEPENDENCY
ROSS SEA
MARIE BYRD
LAND
(Byrd's survey)
King Edward VII
Land
Camp V
Bay of Whales
Little America
Roosevelt I.
Camp VI
Ross Ice Barrier
ROSS SHELF ICE
ROSS
SOUTH VICTORIA LAND
South Magnetic Pole
75°
80°



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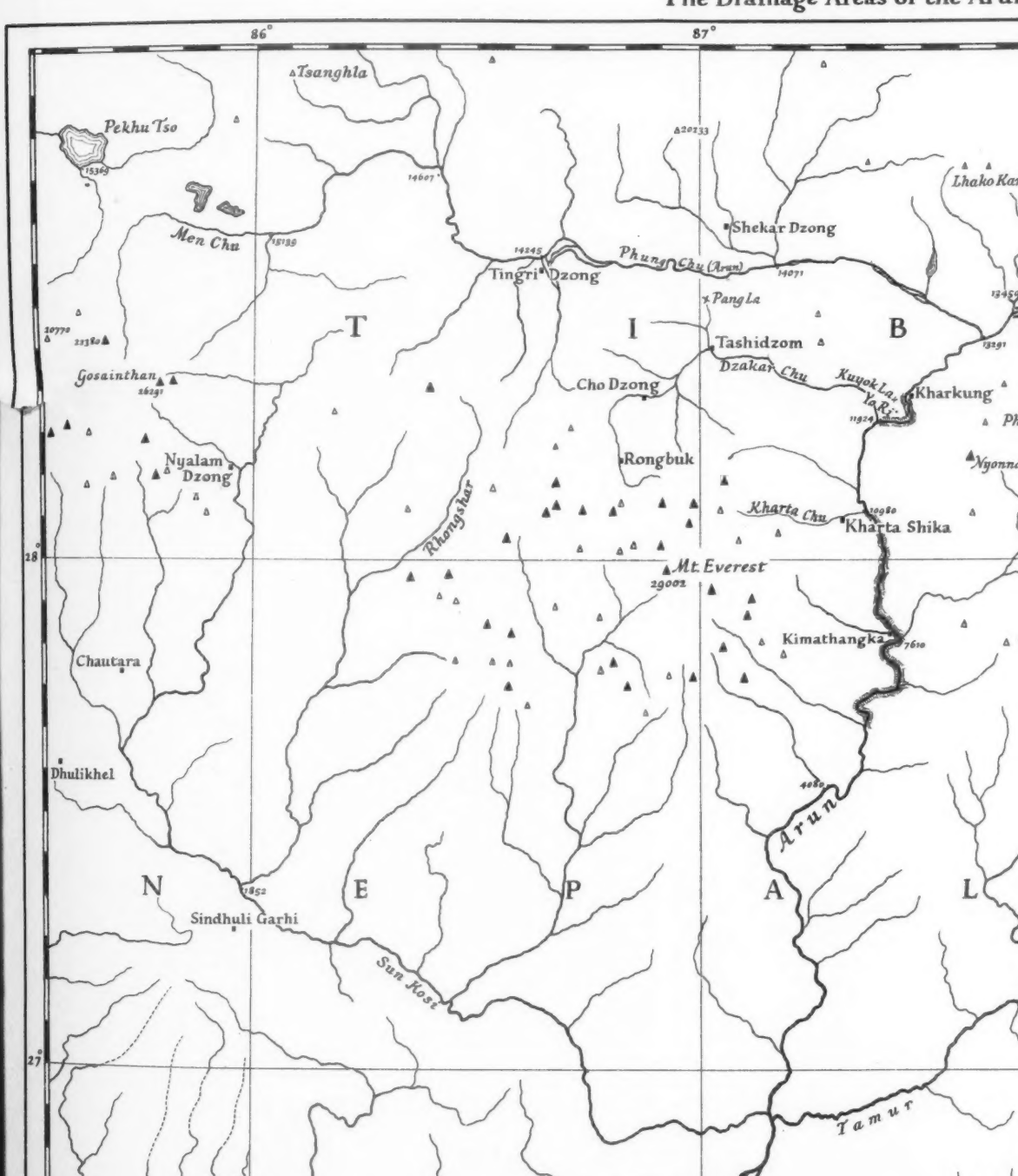
90°
80°
BELLINGSHA
SEA
Ad
Mt. Moniques
Charcot I.
Alexander
1st.
Land
Mairy
C. Pie
Base
FALKLAND
Scr
Finlay
Is.
HEARST
LAND
AND
Mary Louise Ullmer
Weddell
(or Filchner)
Shelf
Ice
te the paper on the
ing of Antarctica"
coln Ellsworth
1935
Lat. 75° 1:12,000,000
Miles
100 150 200 250 300
Kilometres
200 300 400
Photographs (1-12)
Date and Civil Time
ights in feet
ap are those proposed by Mr. Ellsworth
80°

TRANS-ANTARCTIC FLIGHT
Ellsworth

The Drainage Areas of the Arun



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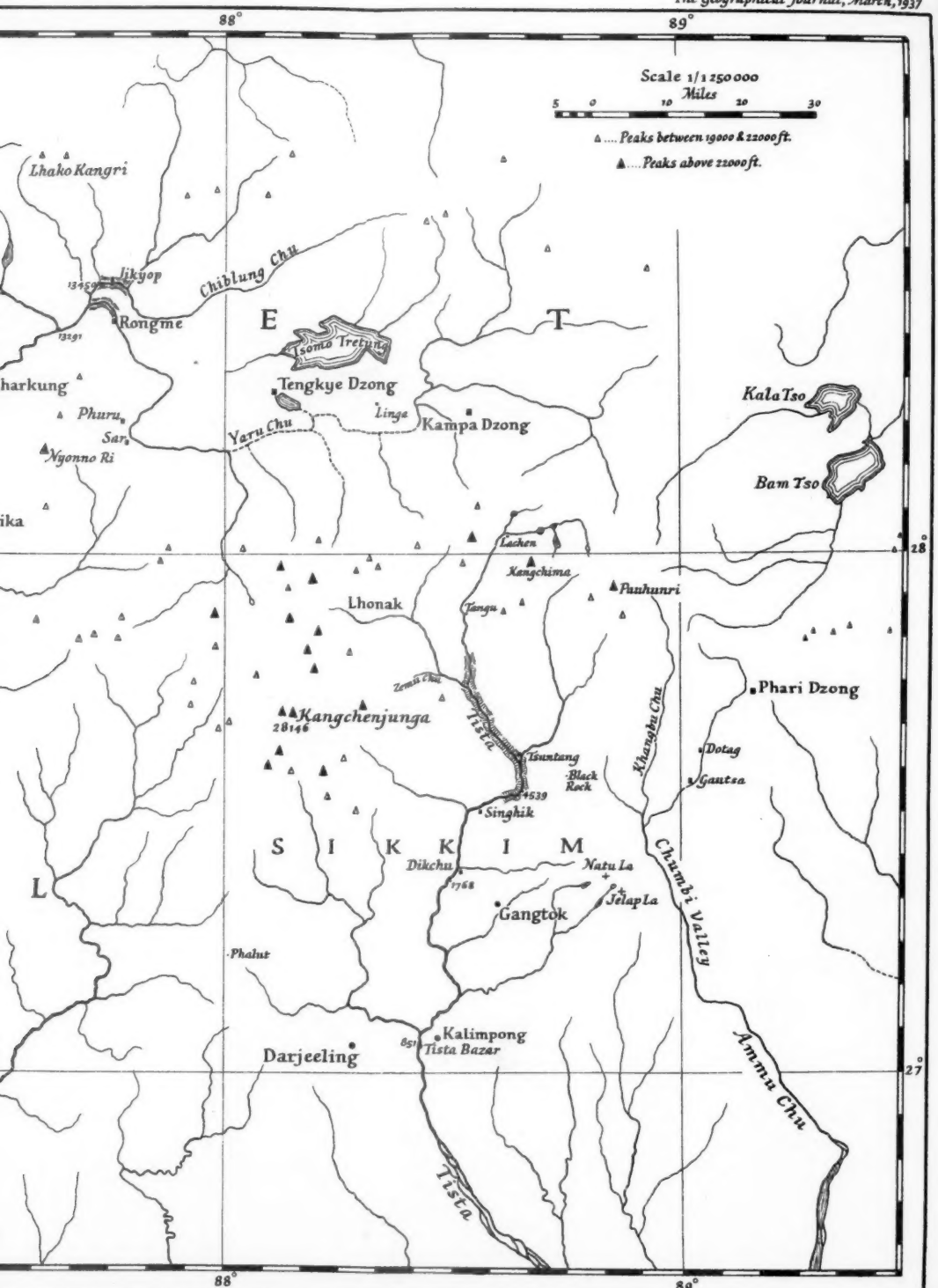
Peaks of Nepal-Tibet Watershed

Everest

Gosainthan

Kangchenjunga

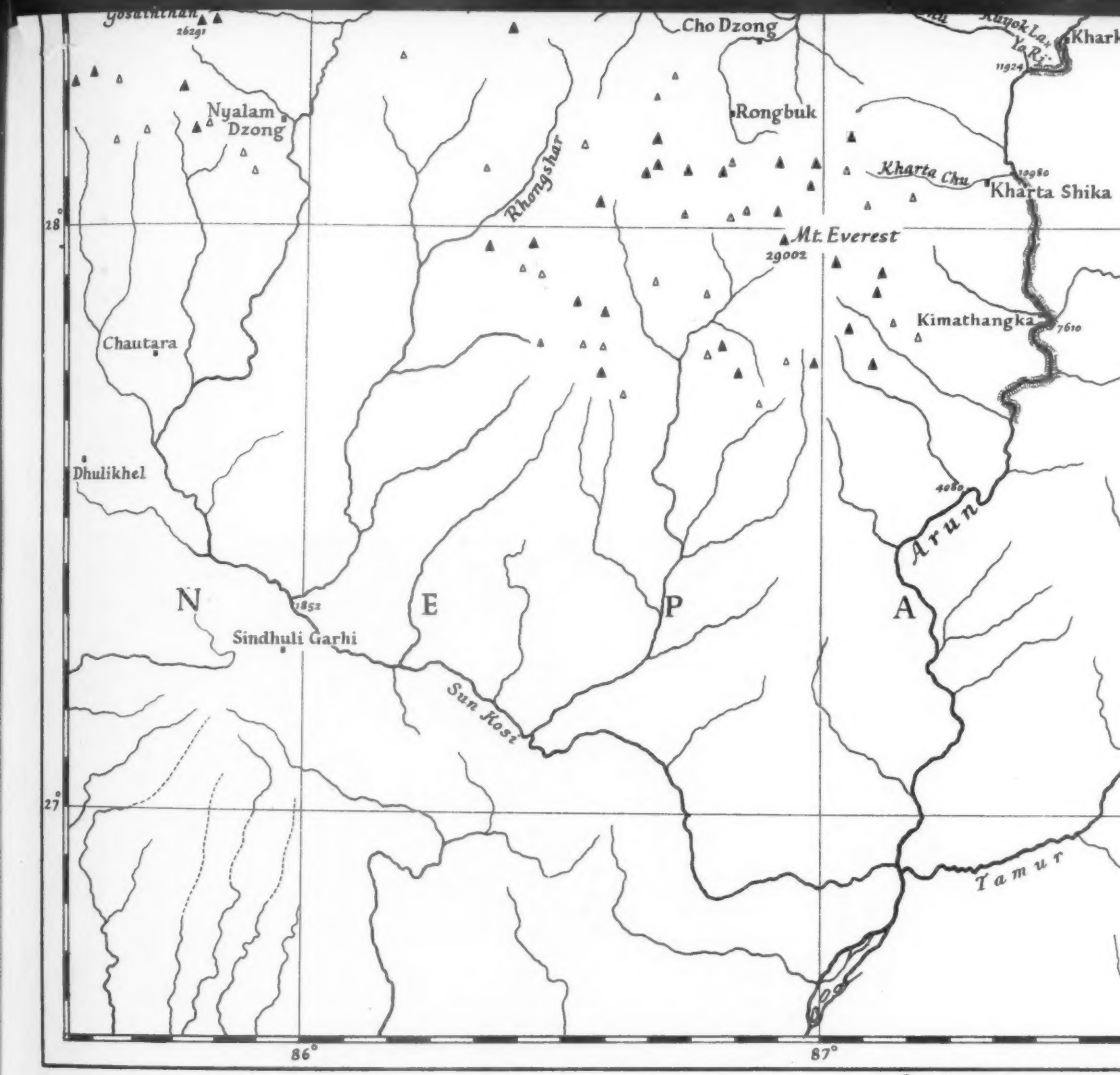


eographical Society

Feet
30000

Kangchenjunga

Kangchenjunga



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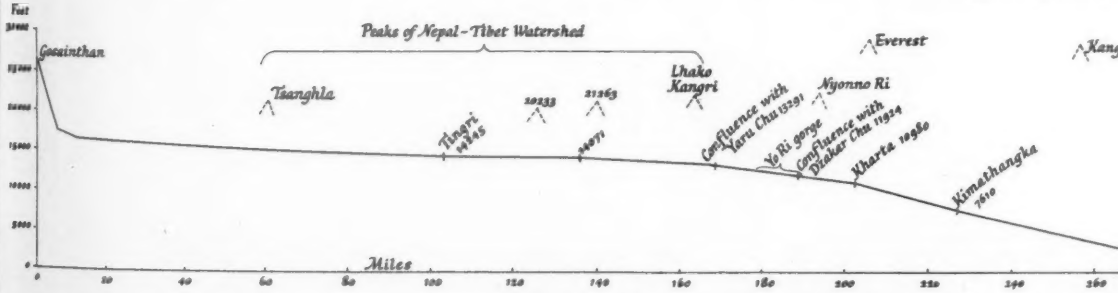
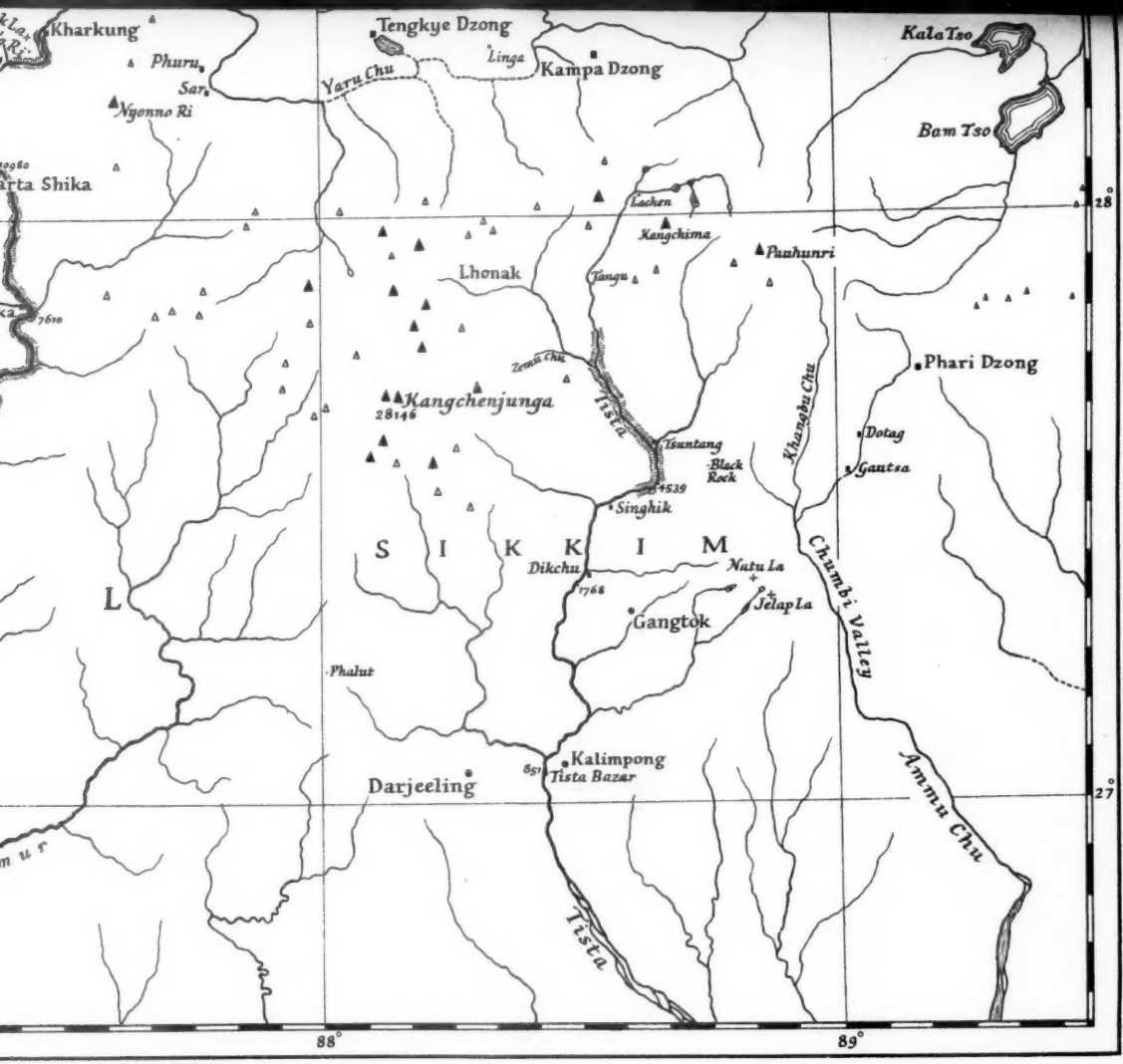


Profile of the Arun in Tibet and Nepal



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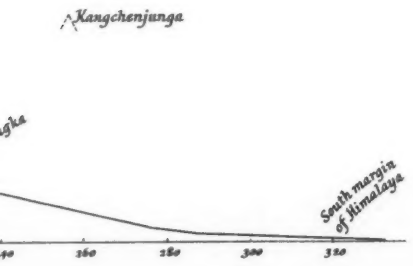

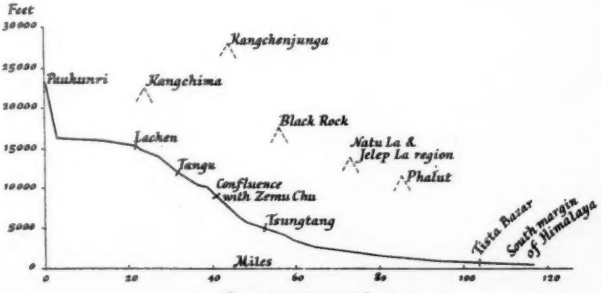

Profile of the Tista

The heights of certain peaks are indicated above the profiles where they are nearest to the valley

ARUN RIVER
Wager